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AUDITORY LOCALIZATION WITH ACOUSTICAL TRANSPOSITION OF THE EARS

BY PAUL THOMAS YOUNG

University of Illinois[1]

If a master surgeon could transplant the right inner ear to the left side of the head, stretching the eighth nerve without disturbing any of the neural connections, and at the same time transplant the left inner ear to the right side of the head, one would expect sound localizations to be reversed as regards right and left. Right-left reversal would be expected from the standpoint of any one of the leading theories of auditory localization.[2]

Although such an operation is out of the question the same result can be brought about acoustically in a relatively simple manner. The right auditory meatus can be extended over the head by means of a sound-proof tube which is connected to an artificial pinna in the region of the left ear; and by a similar device the left auditory meatus can be extended to an artificial pinna near the right ear.

[1] The present investigation was conducted during the summer of 1927 in Berlin, Germany. The work was made possible by the grant of a National Research Fellowship in the Biological Sciences, for which I should like to express my appreciation to the fellowship board of the National Research Council. For valuable suggestions regarding apparatus and procedure I am indebted to various members of the Psychological Institute at Berlin, especially to Professors E. M. v. Hornbostel and M. Wertheimer, and to Dr. Kurt Lewin.

[2] For an introduction to the literature of sound localization see the references given by the following: H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, *Amer. J. Psychol.*, 1922, 33, 178–212; E. M. von Hornbostel, *Das räumliche Hören*, in *Handbuch der normalen und pathologischen Physiologie*, 1926, 11, Receptionsorgane, i, 602–618; E. Meyer, *Stereoakustisches Hören*, in *Handbuch der Physik*, 1927, 8, 538–543; A. H. Pierce, *Studies in auditory and visual space perception*, 1901.

The pseudophone. An apparatus for transposing the ears acoustically is shown in Fig. I. The olive-shaped ear-pieces, right and left, are so made as to fit into the corresponding auditory canals. Each ear-piece is connected by a rubber joint to a sound-proof tube which passes over the head to a receiving-trumpet on the opposite

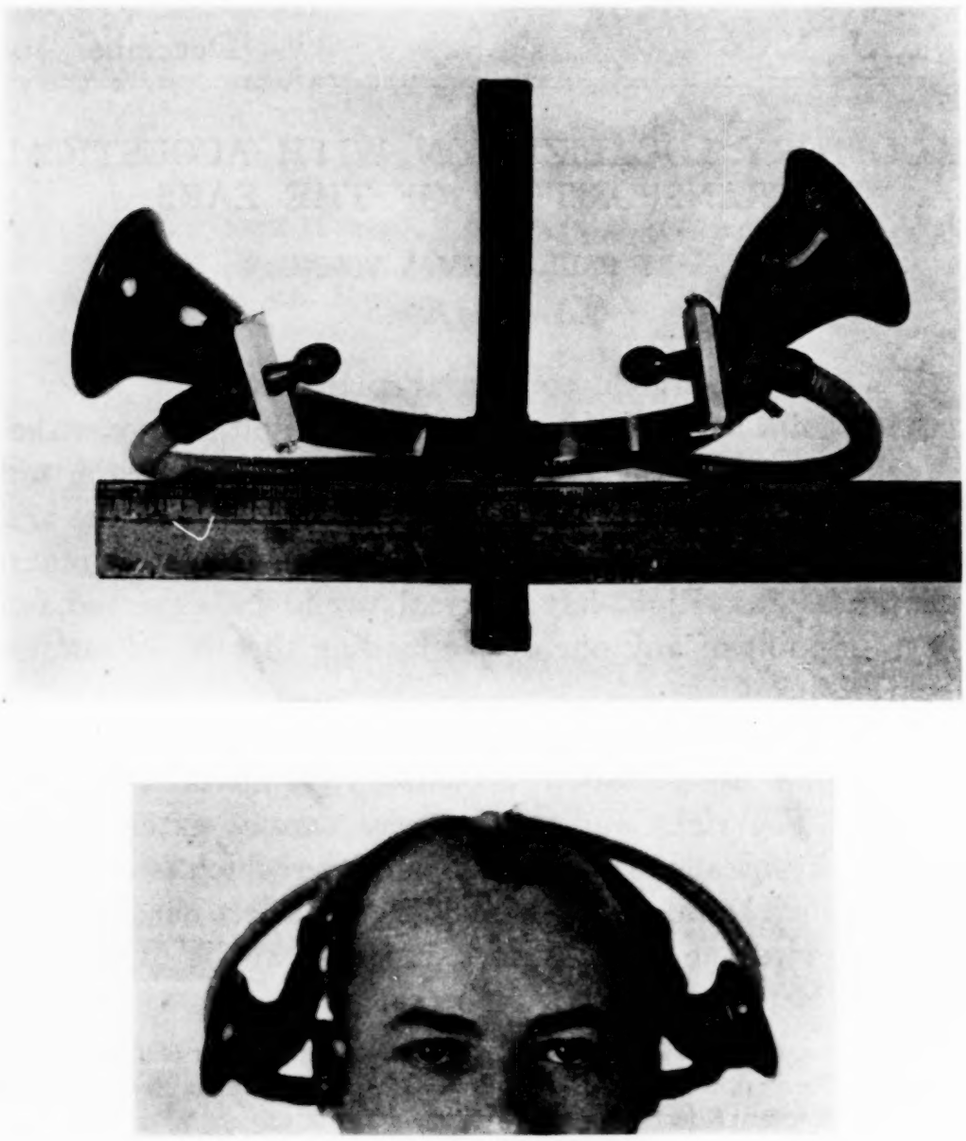

FIG. I
The Reversing Pseudophone

side. These trumpets are made of hard rubber and are taken, in fact, from an instrument designed for persons hard of hearing. All parts are fastened to a light frame which rests snugly upon the head and which exerts a weak pressure on the ear-pieces, thus holding the entire apparatus in place. The receiving-trumpets are placed immediately in front of the natural pinnæ and each is turned slightly forward at an angle corresponding to the natural angle of the pinna.

The tubes of the apparatus are precisely equal in length and are symmetrical in

every respect so that the time consumed by a sound wave in passing from the right trumpet to the left ear is equal to that consumed by a wave passing from the left trumpet to the right ear. Hence the apparatus reverses the normal time-difference between stimulation of right and left ears. It can easily be shown that the apparatus also reverses the phase difference between right and left ears, and further that it reverses the binaural intensive ratio. Consequently right-left reversal of localization is to be expected from the standpoint of any one of the leading localization theories.

Taking the term from Silvanus P. Thompson[3] I have called the apparatus a *pseudophone*. Thompson wrote: "The Pseudophone is an instrument for investigating the laws of Binaural Audition by means of the illusions it produces in the acoustic perception of space. It is therefore the analogue for the ears of the Pseudoscope of Wheatstone, which serves to illustrate the laws of Binocular Vision by means of the illusions it produces in the optical perceptions." The present instrument is wholly different in principle and type from that of Thompson.

The chief practical difficulty with the pseudophone was the prevention of sound leakage between the ear-pieces and the auditory meatus. The slight tension of the apparatus against the ears helped to prevent leakage; but this tension produced headaches (and so had to be reduced) and eventually it made the auditory canal quite sore through friction and rubbing. The ear-pieces were fitted to the writer's ears and when in use they were smeared with thick vaseline. Despite the vaseline there was some sound leakage; but by far the greater part of energy reaching the ears came through the apparatus. When properly adjusted a watch held so close that it touched the skin of the ear was not heard on the same side but was sometimes weakly heard on the opposite side. This watch test was used every time the pseudophone was adjusted and whenever leakage was suspected.

A question which naturally arises is whether the pseudophone modified the characteristics of the stimulus. All sounds heard through the instrument appeared noticeably louder than normal, and this is probably explained by the size and shape of the receiving-funnels which were designed for the deaf and so collected more sound energy than the natural pinnæ. Sounds heard through the pseudophone were accompanied by a faint humming noise similar to that heard when listening to a hollow cavity, and this effect is doubtless due to resonance of the tubes for frequencies of a particular range. The timbre of sounds was also changed by the apparatus; but the modification was slight and did not disturb the observations. With adaptation all of these peculiarities ceased to be noticed.

Experimental

The problem. The experiment started from the assumption that acoustical transposition of the ears would also transpose sound localizations and thus produce a basic discrepancy between visual and auditory spaces. The original

[3] Thompson's pseudophone consisted of a pair of flaps or reflectors fitted to the ears and capable of adjustment so as to catch sounds from back, front, above and below. By varying the angle of the ear-flaps illusions of direction were produced. When, for example, the flaps were so adjusted as to reflect into the ears sound-waves from behind the observer, the localization was in front. S. P. Thompson, The pseudophone, *Phil. Mag.*, (5) 1879, 8, 385–390; (5) 1882, 13, 411–412.

problem was two-fold; (1) What are the characteristics of sound localization with the ears transposed? and (2) Does habituation bring back to normal initial disturbances in auditory localization? The experiment is analogous to Stratton's [4] study of vision without inversion of the retinal image.

First observations

The experiments described in the following account were made mainly by the writer, although a group of nine subjects [5] was used for purposes of control. The observations were made under conditions of every-day life on the streets of Berlin and in the house. Experiments were also made in the laboratory under controlled conditions. In the latter case localization was made by pointing to the source of the sound. Various sources were used such as rapping on wood, ticking of a clock or metronome, whistling and the voice.

First observations of the writer are given in part below. On May 12 the pseudophone was worn about the laboratory for 20 minutes. Disturbances in localization were recorded at the time of their occurrence.

While writing in my note-book the creaking of a door was heard directly behind and then a laboratory assistant was seen entering the door immediately in front (180° reversal). A few minutes later this same assistant stood beside his desk on my left and dictated a letter. His voice seemed louder than normal and it had an unfamiliar timbre. When I looked at him the localization of his voice was entirely normal. The same door opened a second time. At first the creaking sounds were heard in the rear but when I looked at the man entering the room the localization of these sounds changed to front.

The noise of rain pattering on the window pane across the room at my left was distinctly localized off to the right and at the same time a watch seen on a table on my right was heard ticking at the left (double simultaneous reversal). The assistant who stood in front of the window spoke and his voice was normally localized but at the same time the rain pattering on the window was heard on the opposite side (simultaneous normal and reversed localization).

On May 14 I stood before an open window of the Institute and listened to the street sounds in front and a few feet below. The tread of horses on the pavement, auto horns, street-car bells, the hum of motors, etc., seemed to be normally localized.[6] Once a horse came from the left to the median plane. The sound of the tread was normal in localization. When the horse reached the median plane I closed my eyes. Then the horse was distinctly heard to recede in the direction from which he came. A moment later the eyes were opened and when the horse was again seen the localization of the tread quickly became normal.

On May 18 a metronome was placed head-high in a quiet room and started at the rate of 92 strokes per minute. When I stood directly in front of the metronome and

[4] G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Rev.*, 1896, 3, 611–617; 1897, 4, 341–360, 463–481.

[5] The control group consisted of Fl. T. Dembo, Dr. K. E. Zener, Herr K. Dunker, Herr Dr. K. Gottschaldt, Herr F. Hoppe, Mr. N. Maier, Mr. J. F. Brown, Dr. C. R. Griffith and Herr Dr. K. Lewin.

[6] Some of the normal localizations were lacking in precision. The notes record doubt as to whether the ear plays much of a rôle in determining them.

looked at it there was no disturbance in localization. When I turned my right side towards the metronome bringing the source roughly in the binaural axis the clicks were localized at the left despite the fact that I saw the metronome indirectly at the right. Similarly when I turned my left side towards the metronome the ticking was distinctly on the right. The localization was clear-cut and definite and there was an unmistakable discrepancy between visual and auditory localizations of the metronome-object. I walked around the source in a circle of about 1.5-m radius keeping my right shoulder towards the beating metronome. The sound was localized to the left and it moved around the room as I walked. The sound seemed to come from a phantom source upon the wall. I turned my left side to the metronome and walked in a circle in the reverse direction. The sound was distinctly on the right and it moved about the room as I walked.

On May 25 the metronome experiment was repeated. I discovered a curious illusion in which estimations of distance and of direction were incompatible. When my right trumpet was directed towards the metronome the ticking was localized at the end of the room to my left. Keeping my right trumpet towards the source I moved sidewards in the direction of the sound. When I walked towards the sound the intensity became less and less and when I walked away from the sound the intensity grew louder and louder. When I moved towards the sound I appeared to be going in the right direction but at the same time to be getting always farther and farther from the supposed source. When I moved away from the sound I appeared to be coming nearer and nearer to the source. The experience was paradoxical. When I deviated from the direct path I found that the sound was no longer at that end of the room but had changed to some other place. When I walked around the room parallel to the walls the phantom sound moved with me and was always away from the center of the room. The task of approaching and 'finding' the phantom sound-object on the basis of auditory cues was impossible, a will-o'-the-wisp chase.

After the above observations an assistant studied my localizations made with closed eyes. He reported that in the region of the binaural axis localization was misplaced by about 180° but in the region of the median plane another type of right-left reversal was generally made. This second type may be described as right-left reversal without front-back reversal. For example, if the source were 15° to the right of the median plane in front, the localization would be 15° to the left of the median plane in front, and vice versa. The latter type of right-left reversal was also noticed with the control subjects but the conditions of its occurrence have not been investigated.

Definitions. The first observations gave rise to a distinction between normal and reversed localization. *Normal localization* is placing the sound where its objective source exists. This occurs with and without the pseudophone. *Reversed localization* is placing the sound at the subject's left when the source exists at his right and placing the sound at the subject's right when the source exists at his left. Apparently there are two or more types of reversal and these will be considered later. Throughout the following study the terms 'normal' and 'reversed' localization will be used as here defined.

Preliminary Experiments

(*A*) *Binaural intensive ratio.* The sensitiveness of a trumpet may be reduced by stuffing it with cotton. The amount of cotton and the tightness with which it is packed regulate roughly the amount of sound energy passing through a trumpet to the opposite ear. Control of the binaural intensive ratio in this manner gave an interesting and unexpected result.

Sound localizations were thrown towards the more sensitive trumpet. For example, when I stood with my right trumpet turned towards the metronome the clicking was heard at the left. When I stuffed cotton in the right trumpet without changing any other condition the localization remained at the left and was even more definite and distinct than before the reduction of 'sensitivity.' When I removed the cotton and stuffed it in the left trumpet without changing any other condition the first effect was to make localization vague, diffuse, indefinite and this was followed by unmistakable normal localization of the sounds.

This tendency for localization to be on the side of the open trumpet was strikingly demonstrated by the following method. The right trumpet was plugged and the body moved so that at one moment the open left trumpet was turned towards the source and at another moment the open trumpet was turned away. This procedure brought about distinct changes from normal to reversed localization since the localization tended to be on the side of the open trumpet. In rotating the body localization was in one position definitely normal and in another position definitely reversed, but between these two regions were transitional places of indefinite localization.

The observations were repeated at widely separated intervals throughout the entire experiment and always with the same result. As a check the pseudophone was put on backwards so that the right ear-piece fitted into the left ear and both trumpets were directed towards the rear instead of towards the front. A clock was used instead of a metronome. In this case the result was the same; localization tended to be thrown towards the open or more 'sensitive' trumpet.[7]

One very important fact needs to be considered. The above observations were made by the writer alone and with complete knowledge of conditions including the position of the source. One or two tests with control subjects failed to confirm the result when eyes were closed and the subject was ignorant as to which trumpet was stuffed and ignorant of the position of the source. There is need for further experimentation upon this curious phenomenon.

(*B*) *Localization with a single trumpet and ear.* Observations upon monaural localization were made by removing one of the ear-pieces and stuffing the ear with cotton smeared with vaseline. It was found that the index finger covered with vaseline and held in the ear practically eliminated external sounds.

Repeated observations gave the same result. A few excerpts have been made

[7] The result suggests that localizations are normally displaced towards the side of the more sensitive ear. Ferree and Collins, who stress the binaural intensive ratio as a condition of localization, studied observers with ears differing in sensitivity and they also varied the sensitivity by plugging. They found that "(1) Observers having a natural difference in sensitivity of the two ears show a constant tendency to displace the source of sound toward the axis of the side of the stronger ear; and, conversely, observers without this difference in sensitivity show no consistent tendency toward right or left displacement. (2) Changes in the ratio of sensitivity of the two ears, produced by plugging either ear, are followed by corresponding displacements of the sound toward the more sensitive ear. (3) A natural tendency toward right or left displacement can be corrected by making the proper change in the ratio of sensitivity of the two ears." To bring the above result into agreement with our own it is necessary to assume that the right trumpet functions as a right pinna and the left trumpet as a left pinna. This may be the case in visual-auditory localization (definition of this type given later). C. E. Ferree and R. Collins, An experimental demonstration of the binaural ratio as a factor in auditory localization, *Amer. J. Psychol.*, 1911, 22, 294–295.

for purposes of illustration. "Right ear plugged and right trumpet connected to left ear. I am actually hearing with the right trumpet. When the trumpet is turned towards the clock the ticking is louder and clearer than when the trumpet is directed to other places and also the sounds are localized *there* where I see the clock. When the trumpet is turned away from the clock localization is indeterminate. The sounds are nowhere, indefinite, or perhaps they are vaguely placed where the source is. A watch placed next to the right trumpet gives ticking in or next to the left ear." On another day: "Left ear plugged and left trumpet connected to right ear. When the trumpet is directed towards the source localization is normal, exact and certain. When the trumpet is directed away from the source localization is wholly indeterminate.[8] Once I thought it was reversed, but I'm not sure of that."

In general, the results indicate that normal localization is possible with a single trumpet connected to the opposite ear. Normal localization was easy, definite, certain when the clock was located in front of the trumpet. Under present experimental conditions a left trumpet appears to function as a left pinna despite the fact that it leads to the right ear, and a right trumpet appears to function as a right pinna despite the fact that it leads to the left ear.

The above observations were made by the writer with complete knowledge of conditions including the position of the source. On two successive days following these observations, experiments were made without knowledge. The writer was seated and kept his eyes closed. An assistant held the clock in the plane of the head at a distance of about ½ m. Localization was indicated by pointing to the source. The assistant estimated that localizations were objectively wrong in about 80% of the trials. There were some right-left confusions. I found myself observing the loudness and timbre of the ticking sounds and noting how these characteristics of the sound changed when I moved the head. Objectively correct localizations were made on the basis of these cues.

The total experiment shows that knowledge of the position of the source may determine monaural localization; and that experiments with knowledge and without knowledge may give wholly different results.

(*C*) *Observations with two pinnæ on the same side of the head.* On four successive days of the experiment the left ear-piece was removed so that the auditory meatus was unobstructed and at the same time the left trumpet was connected to the right auditory meatus. The arrangement gave two pinnæ—one natural and one artificial—on the same side of the head, and none on the opposite side. The trumpet was a bit in front of the natural pinna and 5 to 7 cm removed from its peripheral ridges (see Fig. I). A similar arrangement gave two right auricles.

A few excerpts from the record follow. "When the two left pinnæ were directed towards the source localization was normal and surprisingly definite. The field of definiteness extended to the median plane and well around to the right in front and in back. When the right side of the head was turned towards the source the localization was indefinite and uncertain and at times localization was lacking. Twice in this case when the head was about a meter from the clock the localization was reversed.

[8] Angell and Fite studied the localizations of a subject deaf in one ear. In the region opposite the deaf ear localizations were extremely uncertain; but on the side of the intact ear they were not seriously defective, and the distinction between front and back was made in a manner superior to that of binaural hearing. J. R. Angell and W. Fite, The monaural localization of sound, *Psychol. Rev.*, 1901, 8, 243.

I tried repeatedly to find the conditions of reversal[9] but failed to do so." "With two right pinnæ localizations are normal in all places around me but at the left there is a region of indefiniteness. Sounds from the left region are voluminous, space-filling, unlocalized and relatively weak in intensity. Occasionally localizations are reversed when the source is at the left and then the sounds have a different *Klangfarbe* from that of normally localized sounds."

Observations were made by the writer with complete knowledge of conditions including the position of the source.[10]

(*D*) *Right trumpet to right ear and left trumpet to left ear.* Following a suggestion of Dr. M. Wertheimer a control experiment was made with modified pseudophone. The apparatus was changed so that the right trumpet connected through the sound-proof tube to the right ear-piece and similarly the left trumpet connected to the left ear-piece. Observations were made without knowledge of conditions; the writer localized and an assistant served as experimenter.

Right and left were never confused but in the median plane front and back were sometimes interchanged. Some of the localizations were made by means of deliberate head movements and observing the changes in intensity and timbre in relation to these movements. When the source was directly in the central axis of the right trumpet localization was due right, although actually this axis pointed somewhat to the front of due right (see Fig. I). The same was true on the left side. Positions in front of the trumpet's axis were confused with positions behind the axis.

The Main Experiment

The aim of the main experiment was (1) to determine the characteristics of sound localization with the ears acoustically transposed and (2) to study the effect of habituation when the ears are transposed.

During the experiment the pseudophone was worn for a period of eighteen consecutive days. For the first nine days the pseudophone was worn an hour daily (11 to 12 A.M., June 10–18). For the following six days the period was lengthened to two hours (10 to 12 A.M., June 19–24). Most of this time was spent upon the streets of Berlin in the region

[9] When a sound wave approaches the two left pinnæ from a point on the left the time difference between stimulation of the ears is little disturbed since the wave passes through the tube about as quickly as it passes around the head. When a sound wave approaches from a point on the right the time difference is reversed and this would tend to give reversal of localization.

[10] While making the above experiment it occurred to the writer that persons who are hard of hearing might find it advantageous to put both ears acoustically on the same side of the head. If the more insensitive ear were placed beside the more sensitive, there would be a double gain; (1) the trumpet would increase sensitivity and (2) both ears could be simultaneously directed towards a source. This plan would increase sensitivity on one side of the head at the expense of the other side. With knowledge of the position of the source, localizations would not be disturbed on the hearing side.

of the Kurfürstendamm, observing the localization of various street sounds.[11] Finally the pseudophone was worn continuously for three complete days. At night during the latter period the pseudophone was removed and the ears were plugged with *Ohropax* (a commercial product) and vaseline. The time (June 25–27) was spent on the street, in the house under every-day conditions and in the psychological laboratory. During the main experiment the pseudophone was worn for a total of 58 hours. From first to last in the present investigation the writer observed with the pseudophone approximately 85 hours.

A pocket notebook was carried and disturbances in localization were recorded when they occurred or as soon thereafter as possible. Publication of the complete record contained in four thick notebooks would burden the average reader with too much detail and repetition. Consequently the results have been summarized in two ways. First, excerpts have been made from the notebooks to illustrate the typical disturbances in sound localization. Second, the record has been studied and described from the temporal point of view to determine the effect of habituation.

(I) CHARACTERISTICS OF SOUND LOCALIZATION WITH THE EARS ACOUSTICALLY TRANSPOSED. In the following sections each paragraph contains material taken from a single experimental day. Within each section the chronological order has been preserved, but a few sample reports cannot indicate temporal relations nor the frequency with which each type of experience occurred.

(*A*) *Dissociation of visual and auditory localizations.* The term 'dissociation' is here used to describe a type of experience in which a well-known source is seen in one place while the sound, known to be associated with the source, is heard in another place.

A mechanical pavement-breaker was making a very loud noise pounding and breaking the rocks on the street. As I approached the machine the sound was clearly and unmistakably transposed in locality. The noise remained reversed regardless of whether my right or left side was turned towards the source. I sat on a bench 150 feet away from the pavement-breaker but the localization was still reversed. By looking steadily at the source and thinking about its position I could get a normal

[11] When the pseudophone was worn on the streets it was covered by a large, loose-fitting cap which left the trumpets and ear-pieces uncovered. The apparatus had the appearance of a rather freakish instrument for a *Schwerhörer*. It was more or less embarrassing to be seen on the streets with the outfit.

localization but I could not get a sharp and definite placing of the sounds at the place of the source.

While thinking out a problem and paying no attention to the auditory experiment I noticed that the clock on my left was heard ticking distinctly on my right and this continued after the abnormality was noted. I was in a state of abstraction.

(*B*) *Right-left reversal of auditory localization.* There were a good many cases in which definite localization was made and subsequent investigation proved that the localization had been reversed. Some examples follow.

I heard a dog barking distinctly to the left-front. Quite automatically I looked towards the spot and saw some shrubbery but no dog. After a random visual search the barking dog was found to the right-back of my original position.

I stood with my back towards the street, looking in a store window. An auto passed behind me from left to right and the localization was normal. The horn tooted unexpectedly first on the left (which was heard right) and then on the right (which was heard left). A small cue such as reflection of objects in the plate glass was enough to give knowledge of actual conditions.

(*C*) *Changes from reversed to normal localization and vice versa.* In a great many cases the reversed localization changed to normal when the position of the source became known. This was frequently the case with continuative noises. Shifts from normal to reversed localization were relatively infrequent but occurred when a source passed out of view and was no longer attended to.

I stood before an open window facing the street which was a bit below. A horse, motorcycle or truck would be heard approaching, for example, from the right and I would expect to see the source appear at the right. Despite expectation the source regularly appeared at the left. When I fixated the source visually and followed it across the field the sounds were normally localized; but after the source had disappeared at the right the sounds were frequently heard receding at the left. Several times there was dissociation: I saw a team walking away to the right and heard a phantom auditory team going away to the left. I closed my eyes and followed the auditory movements by pointing. An assistant said that pointing was consistently opposite to the actual movements of the source. For example, when a vehicle moved from left to right the pointing was from right to left.

At dinner the voices of persons at the table sounded strange in timbre but I soon ceased to notice the peculiarity. Frau K surprised me by speaking while serving at the right. The voice was at the left and then the localization changed to normal. I was distinctly aware of the noises of eating which were localized normally in the mouth and throat regions.

An assistant in the laboratory tested my localizations as I sat on a chair with eyes closed. Unmounted tuning forks of the following frequencies were used: 100, 150, 250, 500, 600 d.v. When a fork was in front of the trumpet localization was on the opposite side. When a fork was moved from right to left localization shifted from

left to right. After this I opened my eyes and held a fork near the right trumpet; localization was to the right. I moved the fork to the left; localization was to the left. Rotating the fork gave the customary maxima and minima of intensity without changing the localization.

(*D*) *Reversal of the localizing reaction.* Not only were the sounds transposed in localization but also bodily movements were occasionally reversed. Bodily movements seem to be secondary processes in the sense that localization does not depend upon them.

While walking along the sidewalk I heard the voices of two ladies and their steps approaching and overtaking me from behind on the right. Quite automatically I stepped to the left making more room for them to pass. I looked back and found that I had stepped directly in front of them. My automatic reaction as well as the localization was reversed.

In the right-back region I heard someone pounding on iron. Quite naturally I turned my head and looked there. Later the source was found in the left-front region: a man on a ladder pounding on an iron pipe one story above the street.

There were repeated cases in which I would expect a truck, horse, street-car or something else to appear in the left (or right) portion of the visual field but, contrary to expectation, the object would appear on the opposite side. In some of these cases I noticed a gross bodily adjustment towards the position of the expected source.

(*E*) *Deliberate correction of the localizing reaction.* Reversals of bodily reaction generally occurred during the early part of the experiment. On the third day deliberate movements were made on the basis of knowledge and not on the basis of the sensory localization.

I found myself deliberately correcting known reversals. I heard a pedestrian overtaking me from behind on the right. Knowing that the person was actually on the left, I stepped aside to the right to let him pass. Once I heard a team of horses drawing near on a side street to my right. Deliberately I looked to the left and saw the team there, and at the same time the localization of the sound shifted.

I heard a pedestrian overtaking me on the left. As the sounds came nearer I expected the person to pass on the right but the sounds were still heard left. For a moment there was an interplay between sensory expectation (left) and intellectual expectation (right). Then I *heard* the pedestrian on the right at the moment he appeared there in indirect vision.

Excerpt from notebook on eighth day: "I have learned that when I hear a sound on the left it is necessary to look to the right in order to see the source, and vice versa. Consequently I sometimes look deliberately in the *wrong* direction and this generally brings the expected source into view. The most natural thing to do is to look towards the place where the sound is heard. Sometimes I follow the auditory cue and sometimes I reinterpret the cue and deliberately look in the opposite direction. I do not mean to imply that I always look or start to look when I localize; this is not the case. In most cases the localization is immediate; it is made before there is any observed bodily movement; the sound is initially heard *there;* movements may be absent or unobserved."

(*F*) *Normal localizations.* One of the most interesting features of the present experiment is the occurrence of normal localizations despite the transposition of the ears. The preliminary experiments showed that, at the very start, fixation of the source, or attention to it, sometimes brought about normal localization. This occurred repeatedly at all stages of the present experiment. Thousands of cases were not recorded.

Excerpt from record of second day: "After the first few minutes the sounds of motor vehicles were localized normally when the source was seen. For example, I watched a street-car approach, pass and recede in the distance; nothing unusual was noticed in the localization of sound. When the object was not seen but its exact position known the localization of sound was also normal."

Thirteenth day: "Most of the localizations were normal for the two-hour period. At the start of the hour and throughout the entire period the sounds of automobiles were normally localized when the vehicle was visible. Street-cars, autos, horses, busses, etc. along the Kurfürstendamm were normally localized when the source was seen. A group of five men were pounding small stones into a side-walk. When I watched them the pounding sounds were normally localized and localization remained normal when I went on. A load of lumber was seen to drop from a wagon to the ground; the loud crash was localized at the place where the wagon stood."

Sixteenth day: "The ordinary house sounds were normally localized—closing of doors, foot-steps, running water, etc. After an hour on the street I had the impression that practically all sound localizations were normal. When an object was seen the sound localization was normal without any thought about it; when not seen but attentively followed localizations were usually normal. Very probably there were undiscovered reversals. . . . When I entered my room the clock was ticking in its normal place regardless of whether right, left, front or back, provided I looked at it or attended to its position. I rested on a couch for half an hour. The clock was ticking at my right and normally localized throughout. Then I put the clock on a table in the center of the room at the height of the head and walked around it. With my left trumpet towards the clock the localization was reversed and the ticking moved around the wall. The reversed type of localization soon gave way to the normal type. When the right trumpet was turned there was at first uncertainty and then normal localization. When I stood with the clock in front the ticking was in back but soon became normal and remained normal. Thus after relaxing on a couch there was at first reversed and uncertain localization and then an appreciable recovery of the normal type."

(*G*) *Atypical auditory localizations.* The record contains three cases of confusion between front and back—two on the left side and one on the right. For example:

An auto was seen to pass on the left. When in front two toots of an auto horn were heard in back on the left. There were no other autos in sight so the case was probably a confusion of front and back on the left side. The localization was immediate.

The record also contains three cases of normal localization made on a purely auditory basis when the source was not in view.[12]

(*H*) *Double localizations.* Several types of double localization occurred.[13] (1) When stimulation was relatively intense localization was sometimes in or at the opposite ear while at the same time a second localization was out in space at the place of the source. (2) When a source moved across the median plane in back there were sometimes two localizations—one to the right and one to the left of the median plane. Both sounds were out in space. (3) When two characteristically different sounds originated at about the same place the sound attentively observed was normal while the other was transposed. This was noticed in the first observations.

The hum of a motor was normally localized and at the same time the hum was also heard on the opposite side. It is hardly correct to say that I heard two hums—one right and one left—for the experience had a unitary character. It would be more nearly correct to say that one and the same motor hum was heard simultaneously in two places.

I sat on a bench with my back towards the street. There were two simultaneous localizations which spread out from the median plane in opposite directions. For example, the sharp sounds made by a horse-tread gave the impression of a horse moving from the median plane to the left and at the same time of a horse moving from the median plane to the right. I followed a team attentively from right to left and checked up visually to make sure that the actual team had passed from right to left. I heard the noises pass normally from right to left. When the team was directly behind there were double localizations. A ghost-like sound which I regarded as an auditory mirror-image moved in a direction opposite to the main sound.

I held a clock in the hand while standing before a mirror. Localization was normal in most places. When the clock was held next to the left trumpet the ticking was heard in or next to the right ear. When the distance between clock and trumpet was somewhat increased the ticking was localized in two places; in the right ear and also out in space to the left. When I brought the clock from a distance gradually nearer to the trumpet double localization was sometimes noticed. This was more obvious on the left than on the right.

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[12] One should consider the possibility that a chance predisposition, or an attitude of expectancy based on knowledge of conditions, determined these atypical cases. Reflection of the sound wave from near-by buildings may also have been involved.

[13] Double localizations have been reported by many observers. Bowlker even reports the existence of triple auditory images. H. Banister, Three experiments on the localization of tones, *Brit. J. Psychol.*, 1926, 16, 267–268; T. J. Bowlker, On the factors serving to determine the direction of sound, *Phil. Mag.*, 1908, 15, 325; Halverson, *op. cit.*, 105, 182 f.

Seventeenth day: I stood in my room with the clock in the median plane about a meter in front of me. I noticed two types of localization: (1) With eyes open, head and eyes fixated upon the clock, an observational set to hear the clock where it is—the ticking was localized in an entirely normal manner. The ticking was *there* in front and there was no other localization. (2) With eyes closed, with a very slight oscillatory movement of the head and with an observational set directed towards the rear, the ticking was localized in the rear. It was unmistakably *there*. The timbre of the sound was different in the two cases. Under the second conditions the sound had a metallic character which was rich and full when compared with the sound heard under the first conditions. Ordinarily there was either one or the other localization but at times both were present together and when this occurred the ticking was heard simultaneously front and back and also there was an unmistakable difference in timbre. The rich metallic ticking was in back and the other ticking was in front.

(*J*) *Unlocalized sounds and indefinite localizations.* When a sound was definitely and precisely localized it was referred to a small spot—whether the localization was normal or reversed. There were times, however, when this spot was large and when the auditory image was poorly defined. There were also times when the localization was merely off-at-the-right or off-at-the-left and the sound seemed to be diffuse and voluminous. Frequently localization was entirely lacking but the loudness, timbre, pitch, and temporal relations of the sound were still definitely recognizable. Unlocalized sounds were very diffuse and sometimes appeared to surround the body indefinitely in all directions. Diffuseness or space-filling character seems to vary inversely with the precision of localization.

I tapped on the pavement and on fence posts with a cane. The localization was not reversed nor was it normal. The sounds belonged *there* at the tip of the cane or on the pavement but there was no clear-cut sensory localization of the sounds. The sounds were referred in thought to the source but they were not heard there. They were not heard anywhere.

Sixteenth day: Once a rumbling sound was heard at the left. The localization was very indefinite. The sound was clearly on the left and it was spread out diffusely in space. Then an auto was vaguely sensed in indirect vision at the right and the localization shifted to the right but it remained indefinite. It was diffuse, in no particular place, but certainly off at the right. Then the auto passed and localization became definite and normal. The sound of the motor was exceedingly loud. . . .

Seventeenth day: An airplane motor hum was recognized and there was a period of visual search for the source. Finally an airplane was seen overhead. The sound which I heard and the airplane which I saw were identified. The sound itself was not localized anywhere; it did not even have an over-the-head character.

Eighteenth day: At dinner voices had a strange, hollow, resonance sound. Once the voices seemed to be floating in the air, but when I looked at the speaker his voice was normally localized.

(II) THE EFFECT OF HABITUATION. On the afternoon of the last day of the experiment an assistant tested the writer's localizations. Eyes were closed and localizations were made by pointing. Every trial gave a right-left reversal! This indicates that habituation to the extent of about 58 hours did not disturb the right-left reversal of localizations made on a purely auditory basis.

Throughout the main experiment when a sound came suddenly, unexpectedly or from an unknown position the localization was reversed. When, in other words, localization was made on a purely auditory basis reversal was the usual thing at the close of the habituation period. The total record contains only three cases of objectively correct localization made under everyday conditions on a purely auditory basis (Sect. G). If one consider that a chance predisposition may account for these three cases, it becomes likely that habituation to pseudophonic localization gave no indication of a return to normal. From first to last purely auditory localizations were reversed.

The case is different when the influence of vision is considered. At the very start of the experiment localizations were occasionally normal when the source was seen or when its position was attentively fixated. This was reported by every one of the nine control subjects who, incidentally, gave reversed localization when tested with closed eyes.

For the writer normal localization usually occurred when the source was fixated visually or when an attentive adjustment was made to its position. As the experiment progressed a casual glance at an object was sufficient to check up the fact that its sound was normally localized. With habituation all of the sounds in a complex situation were normally localized without any thought about the matter. On a busy street, for example, with a street-car *here* and a man talking *there* and an auto passing *yonder*, etc. all sounds were normally localized and the total visual-auditory experience was as it is in every-day life without the pseudophone. And not only this! When a source passed out of view its localization remained normal when I paid any attention to the matter.

In other words, at the start vision sometimes determined the localization and with habituation there was increasing dominance of vision until finally a stage was reached indistinguishable from normal. There was increasing and finally complete visual dominance in determining sound localizations but, as noted above, this did not extend to auditory localizations lacking a visual cue.

Dissociation of visual and auditory localization (Sect. A) was more apt to occur during the first ten or fifteen minutes of the experimental period than at other times. Several of the control subjects also noticed at the start a wholly confused and disorganized visual-auditory world. This does not mean that the occurrence of visual-auditory dissociation was limited to the early part of the experiment. Dissociation was apt to appear at any stage of the experiment when the sounds were loud. It appeared once when I was in a condition of abstraction, and at the close of the experiment dissociation could be brought about through the control of observational attitude when the source was in the binaural axis. But the impression remains—although it is only an impression—that disorganization between visual and auditory worlds was present more frequently during the first than during the latter part of the hour. Moreover, dissociation was more prominent during the first days of the experiment than during the last days. At the close of the experiment it was possible to go for hours without noticing any case of visual-auditory dissociation.

One interesting change with habituation refers not to the place-character of the sound but to the bodily movements. During the first two or three days localizing reactions—moving the head so as to fixate a source, gross movements of the body corresponding to the sound—were occasionally right-left reversed (Sect. D). When this was discovered movements were made deliberately so as to correct the disturbance (Sect. E). Then I practically ceased depending on auditory cues and relied largely on vision. Before crossing a street there was much looking here and there. Habituation led to increased reliance upon vision in the regulation of bodily

movement. Perhaps, however, realization of the unreliableness of auditory cues, combined with the warning of friends that the experiment involved real danger if I were careless, had something to do with this increased reliance upon vision.

The pseudophone was removed at 5:30 P.M. on the eighteenth day of the experiment. There was at first a refreshing calm about the entire world of sounds and this lasted at least ten or fifteen minutes. Doubtless this quiet should be regarded as an adaptation effect. The pseudophone, being made of trumpets for the deaf, increased the intensive level of all sounds and added resonance of its own in a certain frequency range. For three days I had been adapted to this and ceased to notice it. Then sudden relief came and gave the impression that all sounds in the laboratory and on the street were quiet and "soft." Upon removal of the pseudophone localizations were immediately normal and no subsequent disturbance in localization was noted.

The experiment leads to the distinction between two types of sound localization, definable in terms of their conditions; (*a*) auditory and (*b*) visual-auditory. Habituation developed the second type to a point indistinguishable from normal life without changing the first type.

Further Observations

Will continued habituation eventually make purely auditory localizations normal? The above observations furnish no evidence that this would take place, but it is impossible to predict what might happen if the experiment were continued.[14] The reversed type of localization depends upon the habit-systems of a life time. Should one expect these habit-systems to change in a few days?

Possibly one reason why auditory localizations remained reversed throughout the main experiment lies in the fact that the optimal conditions for reëducating the ears were not realized. If vision were entirely eliminated, would auditory localizations correct themselves and eventually change from reversed to normal? In considering

[14] The experiment was brought to a close earlier than at first intended on account of pain in the ear. The constant jarring of the ear-pieces against the auditory meatus made a sore spot which eventually showed a small amount of blood. This became very disagreeable. The pseudophone needs to be made more comfortable for the subject and free, at the same time, from sound-leakage.

this question it is important to notice that a subject experiences no discrepancy and no disorganization when he localizes with closed eyes. A sound is simply *here* or *there* and that is the end of it. The fact that the experimenter regards a localization as reversed may be unknown and irrelevant to the subject. What reason is there to suppose that these 'reversed' auditory localizations would ever change? Unless there is some experienced discrepancy between visual and auditory localizations, or within the auditory world itself, the phrase 'correction of localization' is meaningless. I have no doubt that localizations were made by the writer in which reversal was undiscovered and unsuspected.

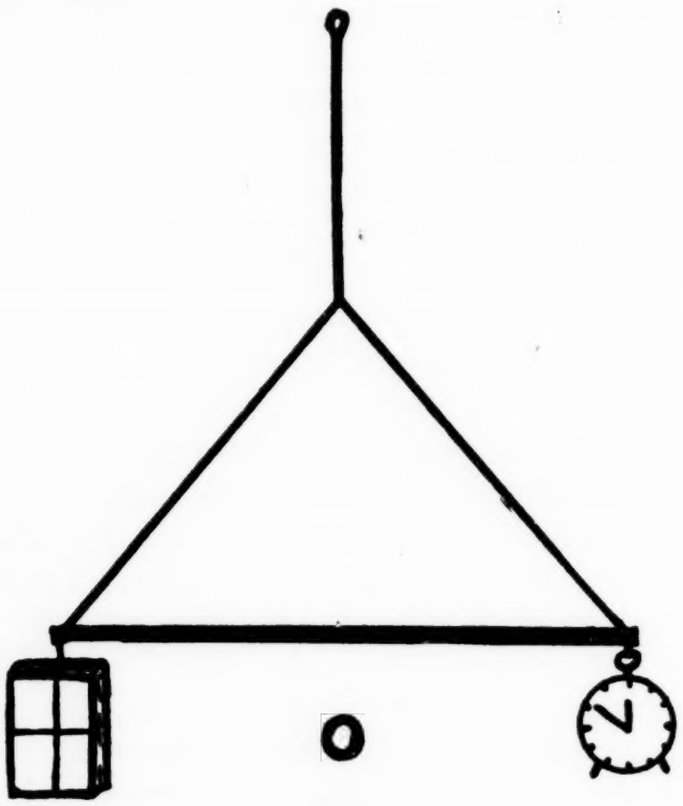

FIG. 2

There were, however, some discrepancies within the auditory world itself. In preliminary observations (see results for May 25) the search for a phantom sound revealed a discrepancy between the estimation of direction and the estimation of distance. Also the direction changed with movements of bodily position; the sound moved with the body much as one's shadow moves when walking around a light. Here, surely, are discrepancies within a purely auditory world!

The situation may be compared with auditory search which is occasionally made in every-day life when the source is unseen and possibly unknown. In seeking a source head and body are moved with an auditory set of observation. Frequently the search is seemingly monaural. The situation of auditory search furnishes the basis for the following simple experiment.

Auditory-motor localization. The aim of the following experiment is to determine whether normal localization can be acquired on a purely auditory basis when the ears are acoustically transposed. The term 'auditory-motor localization' is used to dis-

tinguish deliberate auditory search from the more immediate and unreflecting auditory localization (reversed) described above.

The experimental set-up was both simple and effective. A light wooden beam one meter in length, Fig. 2, was suspended by a cord from the ceiling so that it could be easily rotated. An alarm clock which served as a source was suspended from one end of the beam and a book which served as a counter-weight was fastened to the other. The apparatus was suspended over the observer's head so that the beam cleared the head by about an inch. When the beam rotated the source was carried in a circle around the head at the level of the ears. Movements of the apparatus were noiseless apart from the ticking of the clock.

The procedure was as follows. The eyes were closed and the beam, Fig. 2, put in rotation. Since the angular velocity was variable and since at times the direction of rotation reversed, knowledge of the actual position of the source was soon lost. The beam was then brought to rest and held with the hand in a right-left or in a front-back position. The source was then known to be in one of two possible positions and the problem was to determine on an auditory basis the actual position.

There were 125 trials of this sort, 25 per day for 5 days. The experiments were made on days immediately following the main experiment. Observations were from 11 to 12 A.M.

Auditory-motor search frequently consisted in a gross movement towards the supposed position of the source or away from it. Bodily movement brought an increase or a decrease in the loudness of the ticking and these intensive changes were found to be reliable cues for judgments of the position of the source. Out of the 125 trials objectively correct judgments were made in 120 trials. The 5 erroneous judgments were off-hand and too hasty. I believe that by carefully noting the changes in intensity of sound in relation to movement under present experimental conditions it would be possible to come to correct judgments in 100% of the cases. Mere chance would give 50%.

But the story is not so simple as it seems. In a good many cases there was a discrepancy between judgment and localization. When the source was actually in front the localization was generally in back (reversed). A forward movement of the head gave an increase in loudness and the judgment 'source in front.' Despite this judgment the localization remained in back in 14 out of 16 trials. In the two trials where localization and judgment corresponded there was a deliberate attempt to hear the sound where I knew its source to be. When the source was actually in back in the median plane, on the other hand, the localization was usually in back; judgment and localization agreed.

Limiting consideration to those cases in which the source was either right or left, we find objectively-correct judgments in 96% of the trials, whereas chance would give 50%. In about half of the objectively-correct cases judgment and localization agree (normal localizations) whereas in the other half judgment and localization disagree (reversed localizations). Possibly expectancy is a factor here determining the localization, and expectancy would be equally divided between the two possibilities.

Cases of normal localization made on an auditory-motor basis need further consideration. As the experiment progressed I found I could localize normally without the gross movements of approach towards the presumed source. Slight back and forth head movements as in the gesture of negation were accompanied by changes in the timbre and loudness of the ticking and these changes were adequate cues to judgment. I knew where each trumpet was located on my head and that both

trumpets were directed slightly forward. I moved my head attending to the position of these trumpets. When a trumpet was turned towards the clock ticking was louder than when one side of the trumpet was turned. In addition the ticking was fuller in timbre when a trumpet was directed towards the clock than in any other position.

In making search movements I attended either to the right or to the left trumpet about as I normally attend to the right or the left auricle in searching for a hidden source, and I noticed intensity and timbre changes in relation to movements. It is interesting to note that in 23 cases (in the total 125) normal localization came immediately along with the intensive and timbre change. In 14 additional cases there was at first a discrepancy between localization and judgment and the localization then changed to correspond to the judgment. Several cases of double localization (Sect. H) were recorded.

The experiment shows that judgment of the position of a source and sensory localization are quite distinct and that on a purely auditory basis they may disagree much as they disagree in the visual-auditory dissociations. The experiment shows further that normal localization is possible on an auditory-motor basis despite the transposition of the ears. Normal localization may be dependent upon judgment of the position of the source, but it may also come immediately along with a characteristic timbre and intensity of the sounds.

In considering the above result it should be remembered that the experimenter had knowledge of the nature and construction of the pseudophone and he also knew that the clock was on one end of the beam. If the subject were ignorant of these details, the result might be wholly different.

Transitions between normal and reversed localization. For thirteen successive days following the above experiment an attempt was made to develop normal auditory localization by using vision as a check. I would follow the movements of the ticking clock, Fig. 2, with eyes open and then continue to follow with eyes closed. Sometimes I would start with closed eyes and then determine the position of the source by peeking.

The result of the attempt was negative: normal auditory localization was not acquired through the method, and there was no indication that it might be acquired in this way.

Unexpectedly, however, the experiment revealed some interesting transitional experiences. These are illustrated by the following excerpts from the record.

At the beginning of the hour I kept my eyes open and followed the revolving clock fixedly. Despite this there was dissociation: the visual clock was at one place and the ticking consistently 180° from that place. Then I strained and suggested to myself that the ticking was at the source; but despite this I did not hear it there. Presently I noticed that when the clock rotated directly in front of the right trumpet (near the axis of the trumpet) the localization was unmistakably normal. When the clock was outside of this narrow region localization was reversed and dissociated from the visible

source. For two or three revolutions there was a shift from reversed to normal localization and vice versa. When the clock was in front of the trumpet localization was normal and when outside of this region it was reversed. The phenomenon was so striking that I stopped the bar and moved my head as in the gesture of negation. The localization shifted from reverse to normal and vice versa as the clock came in and out of the critical region before the right trumpet. When localization was normal the sounds were louder than when reversed; also there was a timbre difference. Timbre and intensity changes went along with the changes in localization as they did in the experiments upon auditory-motor localization. This condition in which localizations were normal in some regions and reversed in other regions did not last. With repeated observation the region of normal localization widened to the front and back. Then I discovered a region of normal localization in front of the left trumpet. Both regions of normality spread until after a few minutes I was localizing normally all around the circle. The above type of transition was observed on at least three experimental days and always at the first of the hour.

At first the eyes were kept closed and localizations were made with an auditory set of observation. I followed the moving clock; its auditory position was definite and precise. Occasionally I opened my eyes for an instant and found that in every case the localization was reversed. This peeking apparently did not disturb the localizations. After a few minutes I opened my eyes and kept them open. Immediately there was dissociation: the clock was seen in one place and the ticking was heard 180° from that place. After a few revolutions of the apparatus I fixated the moving source and followed its movements. When I did this localization became indefinite; the ticking was heard but it was nowhere. It would be impossible to describe the localization as either normal or reversed. I identified the ticking and the clock intellectually; I knew they belonged together but at the same time there was no clear-cut localization of the sound. Later localization became normal.

On another day I commenced observations with a visual set. After a few minutes localization was normal in all positions. At first normal localization demanded a constant following of the source or attention to its position. Later normal localization became easy and direct. When normal localization was established I closed my eyes and continued to follow the movements of the source. Several times there were two localizations of the ticking—one normal and one reversed. I noticed strains on the top of my head, towards the occipital region, around the ears, and strains of eye movement when I attempted to hear normally with closed eyes. It was possible to localize normally for a few revolutions; this was checked by peeking. Presently the observational strain relaxed and localization slipped back to the reversed type.

Summary and Discussion

Effect of the pseudophone upon auditory localization. The present experiment started from the assumption that auditory localization depends upon a difference of stimulation between the right and left ears. The physical nature of this difference has not been considered: it may be a difference of time, phase, amplitude, wave-form, or a combination of these conditions. Whatever the essential physical basis of locali-

zation may be, the acoustical interchange of the ears may be expected to give a right-left reversal to sound localizations.

The investigators of normal auditory localization have noted that right and left are distinguished with great accuracy. Thus Preyer[15] wrote: ". . . und zwar ist unter mehreren tausend Versuchen an normal hörenden Menschen nicht ein einzigesmal ein von links kommender Schall nach rechts verlegt worden und umgekehrt." And Matsumoto:[16] "No sound on the right side was perceived as being on the left side, and no sound on the left side was perceived as being on the right side." And von Hornbostel[17] regards it as established that: "Rechts und Links werden in der Regel nie verwechselt, Vorn und Hinten, Oben und Unten häufig." On the basis of such statements one would expect the acoustical transposition of the ears to give highly consistent right-left reversals in localization.

The theoretical expectation has been abundantly confirmed. When localizations were made with closed eyes and without knowledge of the position of the source there was a consistent and unmistakable right-left reversal of localizations. This was the case with a control group of nine subjects. With the writer, reversed localization persisted after the pseudophone had been worn about 85 hours. With the acoustical interchange of the ears a source on the left was placed with great regularity on the right and vice versa. The few exceptions to this rule can be explained by reference to extraneous experimental conditions.[18]

The exact effect of the pseudophone upon auditory localization has not been studied, and it is clear that a thorough quantitative investigation is needed to clear up the matter. In general, two types of right-left reversal were noted.

(A) The most frequently noticed disturbance was a right-left reversal of about 180°. When the source was 60° from the median plane in the right-front quadrant, Fig. 3, the localization was 60° from the median plane in the left-back quadrant. When the source was 90° left the localization was 90° right and vice versa.

[15] W. Preyer, Die Wahrnehmung der Schallrichtung mittelst der Bogengänge, *Pflüger's Arch. f. d. ges. Physiol.*, 1887, 40, 595.

[16] M. Matsumoto, Researches on acoustic space, *Stud. Yale Psychol. Lab.*, 1897, 5, 5.

[17] *Op. cit.*, 604.

[18] See Sect. G above and footnote 12. Further exceptions were found with three control subjects who localized sounds in the rear when the source was due left. The observations have been disregarded on the ground that sound leakage was not adequately controlled.

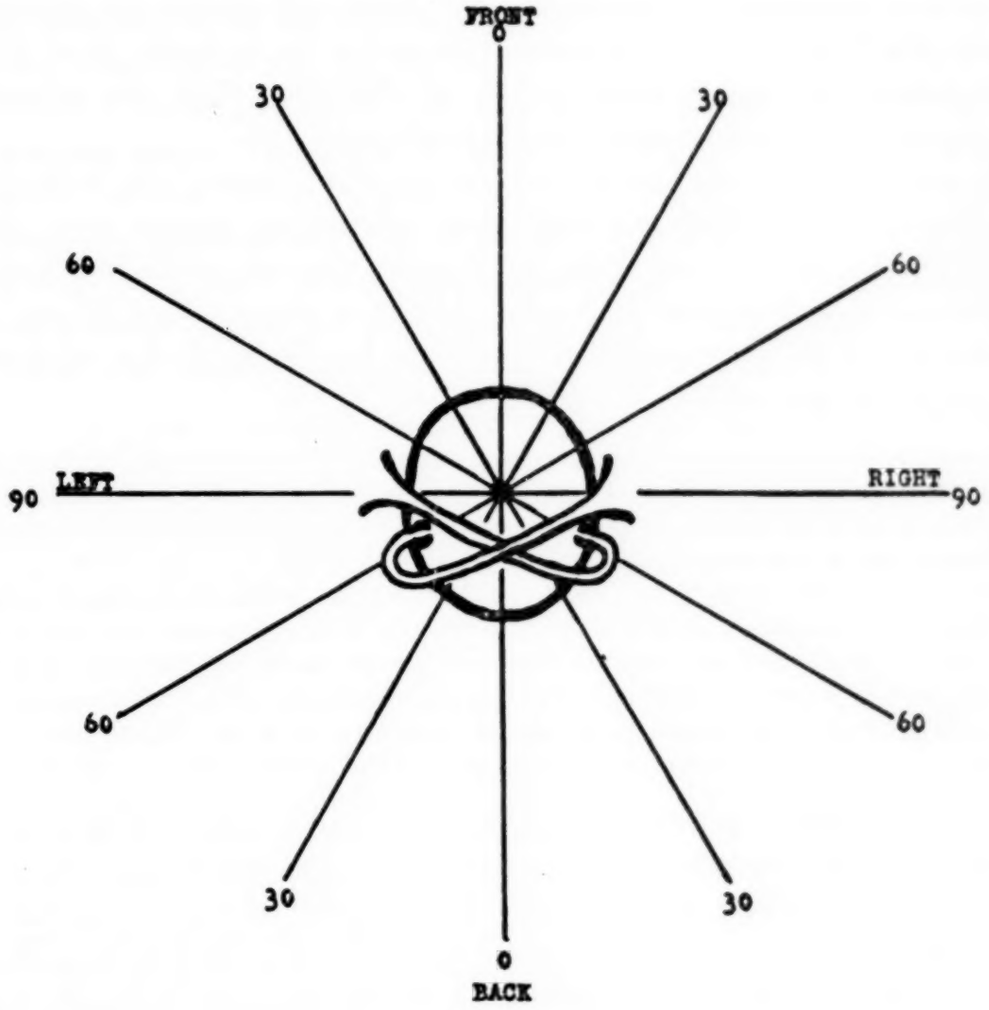

FIG. 3

The relationship is not exactly one of 180° as will be seen from the following observation. Let S represent a source, O an observer, and L the position of the localized sound.

Ṡ Ȯ L̇

If the relationship between source and localized sound were exactly 180° in every case, rotation of the body of O would not change the apparent position of L. The experiment was made several times by the writer. In four critical positions the 180° relationship was seemingly exact; when the source was due right or due left and when it was in the median plane in front or in back of O. Between any two of these positions the sound was displaced to the right or to the left;

in two quadrants it moved to the right and in two quadrants to the left. This observation is given to indicate that the relationship is not exactly one of 180° and that the matter needs to be investigated quantitatively.

(B) On several occasions an assistant tested the writer's localizations. He reported that when the source was, for example, 10° to the right of the median plane in front the sound was localized 10° to the left of the median plane in front. The same relationship was found near the median plane in the rear.

When standing before a window facing the street this type of right-left reversal was sometimes noticed. It was also observed when sitting on a bench with my back towards the street. In these cases, however, knowledge of the position of the street may be a determining factor.

A clock was carried in a circle around the head of one of the control subjects who had been instructed to follow its motion by pointing. When the source was 30° right-front he pointed 30° left-front (see Fig. 3); when the source was 60° right-front he pointed 60° left-front; when 90° right he pointed 90° left; when 60° right-back he pointed 60° left-back, and so on for several complete revolutions. This was rare for most frequently he and the other subjects pointed to a position 180° from the source.

The median plane presents a special case. When the source is due front it is generally localized as due back. When the source is due back it may be localized in front but most frequently it is localized in back. In other words, localizations in the median plane tend to be thrown towards the rear. This suggests the normal tendency to throw localizations forward.[19]

The above statements are not intended to represent final conclusions but rather to indicate problems in need of further study. Right-left reversals should be studied in relation to such conditions as the distance between the trumpets, the direction in which the trumpets face, the binaural intensive ratio, etc.

Visual dominance. Acoustical transposition of the ears introduces a basic discrepancy between sound localization and visible source. When, however, a source is fixated visu-

[19] Geissler writes that "this predominance of the front half over the back half in expectation and localization of sound seems to be due to the great influence of vision upon the whole of mental life." L. R. Geissler, Sound localization under determined expectation, *Amer. J. Psychol.*, 1915, 26, 268–285.

ally or merely attended to without fixation the sounds originating from the source may be localized normally. This was observed repeatedly by the writer and verified by a group of nine control observers. Habituation to pseudophonic hearing made the normal localizations more frequent, certain, and definite until finally the visual-auditory integrations were indistinguishable from those of ordinary life. At the close of the habituation experiment the writer could remain on a busy street for an hour or more without noticing any lack of coördination between visual and auditory experiences.

The present experiment is related to the work of Professor Stratton. He wore an optical apparatus made of two double convex lenses so placed as to rotate the retinal image (monocular) through 180°. This introduced a discrepancy between visible source and localization of the sound involving not only an interchange of right and left but also of up and down. Although Stratton's interest in the problem of sound localization appears to be rather secondary, his results, so far as they go, confirm in every respect our own. A few pertinent quotations follow. "Localization of sounds, when the source was in sight, followed in most cases the visual position of the source, provided I did not voluntarily recall the older position of the object." ". . . the sound of my steps seemed to come from the place where I felt my feet strike—in this case a divergence of 180° from the old direction of the sound." "Localization of sounds varied, being different when the source of sound was in sight from what it was when this was out of sight, and also in the latter case differing with the different directions of attention, or with the suggestions as to the direction from which the sound came. . . ." In Stratton's experiment the visual field was turned around; in the present experiment the auditory field was reversed; in both experiments there was visual dominance in the determination of sound localizations.[20]

In every-day life one does not have to search far to find situations in which vision dominates in determining sound localization. The ventriloquist speaks consistently in one voice when he moves puppet A and in another voice when he moves puppet B. The on-looker becomes gradually adjusted to the situation and then gets the well-known ventriloquist's illusion. The vitaphone (talking motion pic-

[20] Stratton, *op. cit.*, 360, 467. A discrepancy between auditory and visual localization under conditions somewhat similar to those of the present experiment was also reported by von Hornbostel and Wertheimer. The apparatus consisted of two large receiving funnels separated by two meters and mounted on a standard with tubes connecting the funnels with the ears. When the tubes were crossed dissociation was reported. "Verkreuzt man z.B. die Schläuche, die von den Trichtern einer 2-m-Basis zu den Ohren führen, so kann man jemand in 60° links hören, während man ihn in 5° rechts stehen sieht." E. M. von Hornbostel und M. Wertheimer, Über die Wahrnehmung der Schallrichtung, *Sitzber. d. preuss. Akad. d. Wiss.*, 1920, 20, 388.

tures) gives a further illustration of the same principle. The voice of a speaker in an auditorium may be indefinitely localized but fixation upon the speaker makes the localization more definite. Here are cases of visual-auditory localization!

Auditory and visual-auditory localization. Although visual-auditory localizations developed so that the sight-sound coördinations were indistinguishable from those of normal life, auditory localizations remained persistently reversed till the close of the experiment. Both types of localization are stable and they are easily distinguished since one type gives reversed and the other type gives normal localization. Auditory localization depends upon some physical difference between the stimulation of right and left ears. Visual-auditory localization depends upon an adjustment of the organism with respect to a visible or known position in phenomenal space.

The experiment as a whole makes it necessary to distinguish between these two types of sound localization. And the distinction relates in an orderly way all of the facts revealed in the main experiment: dissociation of source and sound; right-left reversal of localization; shifts from reversed to normal localization and vice versa; reversed localizing reactions; normal localizations; double localizations. Indefinite localizations and a few atypical localizations can be explained as intermediate or transitional experiences.

Auditory-motor localization. When a deliberate search is made for the source of a sound without the aid of vision and touch subjective changes in the loudness and timbre of the auditory experience in relation to head and gross bodily movements assume the leading rôle. Auditory-motor localization may lead to a correct judgment of the position of the source. It may also lead to an immediate normal localization despite the reversal of the pinnæ.

How are sounds localized? The physical theories are doubtless correct in maintaining that sound localization depends upon some difference between the stimulation of the right and left ears; but these theories do not go far

enough.[21] They throw no light on the physiological and psychological processes of localization. They fail to take into account the possibilities of visual and auditory-motor localization.

Equally incomplete is the statement that sound localization depends upon 'suggestion' and 'knowledge.' What is 'suggestion'? and how does 'knowledge' operate? We have already seen that sensory localization—the hearing of a sound as here or there—may not agree with the knowledge of the position of the source.[22] Words cannot solve the problem as to how sounds are localized.

Animals react to sudden noises by moving the body so that the source comes to be visually fixated. If a noise is made near a resting dog, the ears will be pricked up and the head moved so as to bring the source before the eyes. The writer recently saw a cat pass beside a basement window at a distance of about two meters. When the animal was directly opposite the window he tapped lightly on the pane. Instantly the cat stopped, turned her head towards the window, looked, and ran away. Münsterberg[23] has pointed out that such localizing reactions have biological value since they bring the source before the eyes and under the nose of the creature.

Infants, during the early months, localize sounds by turning eyes, head and body so that the source comes to be visually fixated. Koffka[24] has summarized the situation in the following words: "In the beginning specific reactions to auditory stimuli are lacking, but during the third or fourth month—sometimes even in the second month—a response is developed that appears to be like the eye-movements of fixation, when the infant turns his head in the direction of sound." This was observed by Preyer,[25] Shinn,[26] Moore,[27] and others.

Preyer, Münsterberg and others have maintained that the reflexes of sound

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[21] Stewart writes: "Satisfactory explanations of the many phenomena in sound localization can be realized only after the bases are laid for a successful discrimination between physical and psychological factors." I believe that the pseudophone provides a simple means of discriminating between those conditions which reside in the stimulus and those which reside in the organism. Further, the pseudophonic technique may well be used in studying the relative importance of intensive ratio, time difference, phase difference, head movements, etc. These factors can be controlled and varied more or less independently. See G. W. Stewart, Phase relations in the acoustic shadow of a rigid sphere; phase difference of the ears, *Physical Rev.*, 1914, 4, 252.

[22] Stratton makes a similar distinction when he observes that "things were thus *seen* in one way and *thought of* in a far different way," *op. cit.*, 613.

[23] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 1889, Heft 2, 204–205.

[24] K. Koffka, *The growth of the mind*, 1924, 82.

[25] W. Preyer, *The mind of a child*, 1890, Pt. I, 85.

[26] M. W. Shinn, Notes on the development of a child, *Univ. of Calif. Publ.*, 1893–1899 (reprint 1909), 1, 109.

[27] K. C. Moore, The mental development of a child, *Psychol. Rev. Monog. Suppl.*, 1896 (no. 3), 66.

localization depend upon stimulation of the ampullar nerves. Preyer[28] writes: "Es ist also eine völlig legitime Hypothese, wenn ich behaupte: die specifische Energie der Ampullennerven ist es, ein mit Schall verbundenes Raumgefühl zu geben, und zwar ein Richtungsgefühl." And Münsterberg[29] states that "eine Lokalization der Schalleindrücke wäre einfach unmöglich, wenn nicht jeder Schall, je nach der Richtung, aus der er kommt, mittelst bestimmter Bogengangerregung eine bestimmte Kopfbewegung hervorrufen würde, welche, auf demselben Weg wie jede Bewegung durch Muskelempfindung zur Wahrnehmung gelangt." "Einen Schall lokalisieren, heisst, die Empfindung derjenigen reflektorischen Kopfbewegung, welche notwendig ist, um sich der Schallquelle zuzuwenden, einordnen in das gesamte System der Kopfbewegungsempfindungen." These views have not been taken very seriously by psychologists. In the present case the semicircular canal theory is not adequate to account for the non-auditory types of sound localization and for various other facts of the main experiment.

Actual movement appears to be unnecessary for sound localization. I believe that the essential condition is a neuro-muscular set,[30] a postural adjustment of muscles relative to some position in space or relative to the total situation. This set is the condition of movement when localizing movements do occur, but the set may persist without any explicit reaction.

What is meant by neuromuscular set may be demonstrated as follows. Let the reader pick out certain points in front, in back, and to the side; then with eyes closed let there be attentive fixation first on one point and then on some other. The kinæsthetic pattern in the region of the eyes and head will be felt to shift with change of attentive fixation. Doubtless the kinæsthetic patterns indicate changes in the tonus of muscles. In the early stages of the main experiment visual-auditory localization seemed to involve similar adjustments with reference to the position of the source.

Goldstein [31] has noted that *Einstellung* may displace sound localizations in a non-conscious, physiological manner. He

[28] *Op. cit.*, 611, see footnote 15. A semicircular canal theory of space has been developed by von Cyon. The three canals are supposed to be related to the three dimensions of geometrical space. E. von Cyon, *Das Ohrlabyrinth als Organ der mathematischen Sinne für Raum und Zeit*, Berlin, 1908.

[29] *Op. cit.*, 197.

[30] P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 1925, 32, 472.

[31] He has also reported that tonal localization depends upon eye-movements both in the case of normal individuals and with those suffering from brain lesions. K. Goldstein, Ueber induzierte Veränderungen des 'Tonus,' *Schweiz. Arch. f. Neur. u. Psychiat.*, 1926, 17, 210, 216, 225. See also K. Goldstein & O. Rosenthal-Veit, Ueber akustische Lokalisation und deren Beeinflussbarkeit durch andere Sinnesreize, *Psychol. Forsch.*, 1926, 8, 318.

has also shown that localization should be regarded as a function of the total organism. "Jeder sensorische Reiz beeinflusst jede andere gleichzeitig vorhandene Wahrnehmung in ihrer Lokalisation." Patients with cerebellar and frontal lesions showed disturbances in sound localization but they also showed similar disturbances in cutaneous and visual localization when the test was made by pointing. Localization depends upon muscle tonus.

The present experiment has made it clear that sound localization is not merely a function of the ear. It is an accomplishment of the organism as a whole involving muscle-systems common to both eye and ear. Under ordinary conditions auditory localization and visual-auditory localization bring about the same result—visual fixation of the source—and they bring about this result through the same muscles—those that move the eyes, head, trunk, etc.

With the pseudophone, auditory localization and visual-auditory localization became divorced and opposed in their demands upon the localizing musculature. The types remained distinct and the experiment gave no evidence that the auditory type is a derivative of the visual-auditory type. The probable explanation of this negative result lies in the fact that the two types of localization depend upon wholly different stimulus conditions. Auditory localization depends upon some acoustical difference between stimulation of right and left ears. Ordinarily the stimulation changes muscle tonus so that if movement results, the subject's eyes are directed towards the source. Visual-auditory localization, on the other hand, depends upon some visual cue which determines at the start the position of the source, and this cue may also determine the localization of the sound. The position of the source may be determined by touch and even by sound (auditory-motor localization). It is a neuromuscular set or adjustment towards the source or the total situation which determines sound localization, and this set is describable as a pattern of muscular tonus.

If the above analysis is correct, there is no reason to expect that further habituation to visual-auditory localization with

the pseudophone would eventually develop normal auditory localization. The former depends upon non-auditory cues and when these cues are removed there are left merely the acoustic conditions of localization and with the pseudophone these conditions result in reversed localization.

Whether repeated *auditory-motor* localization would eventually bring about normal *auditory* localization is a question which can not be answered on the basis of present results.

Volume and localization. Boring [32] regards volume as ". . . at the basis of auditory localization just as visual and tactual extent are basal to visual and tactual localization." Banister [33] considers Boring's statement as improbable and points out that there is some uncertainty concerning the attribute of volume. "Hence, until further evidence to the contrary is adduced, one might be justified in maintaining that 'volume,' as an attribute of sounds, arises (*a*) from our tendency to objectify our experiences, combined with (*b*) the difficulty experienced when we endeavour to localise the source of the sound." We tend to reify, to impute to sounds the attributes of objects.

In the present experiment volume seemed to vary with the precision of localization. Sometimes a sound was heard precisely at a single spot. At other times the sound was larger and visualized as covering an area. Sometimes a sound was vaguely off-at-the-right or off-at-the-left and it appeared more diffuse than the definite localizations. Most interesting of all were the unlocalized sounds in which the pitch, intensity and timbre were about as always but the sounds had no particular place character. Sometimes they surrounded the body diffusely.

Unlocalized sounds appeared to be transitional between the two opposed localization types. A leak artificially made around the ear-pieces was found to make localizations indefinite, to give them greater outspread.

[32] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *Amer. J. Psychol.*, 1926, 37, 176.

[33] H. Banister, Auditory theory; a criticism of Professor Boring's hypothesis, *Amer. J. Psychol.*, 1927, 38, 439. See also G. J. Rich, A preliminary study of tonal volume, this JOURNAL, 1916, 1, 13 f.

The observations suggest that volume and precision of localization are inversely related; the greater the volume the less precise the localization, and vice versa. In a good many unlocalized sounds, however, the volume was not observed.

I do not know what relation these observations may have to the views of Boring and Banister but I believe that volume should be more carefully studied in relation to localization.

Conclusions

1. Acoustical transposition of the ears by means of a pseudophone produced a right-left reversal of auditory localizations.

2. Despite the transposition of the ears, localizations of sound were normal when the source was seen or its position attentively fixated.

3. There are at least two distinct types of sound localization; (*a*) auditory localization and (*b*) visual-auditory localization.

4. Habituation up to 58 hours did not modify auditory localizations and systematic practice up to 13 additional one-hour periods gave no indication that normal auditory localization could be developed from the visual-auditory type.

5. Habituation to visual-auditory localization with the ears acoustically transposed developed the type until indistinguishable from normal.

6. Deliberate movements of search made with eyes closed in relation to observed changes in the loudness and timbre of sounds lead to objectively correct judgments of the position of the source and occasionally to immediately normal localizations, despite the acoustical transposition of the ears.

7. Localization is believed to be a function of the total organism, accomplished in various ways, but involving a neuromuscular adjustment with respect to a source or to a total spatial situation. The physiological conditions of localization are to be sought in pattern changes of muscle tonus.

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THE PSYCHOLOGY OF CHINESE CHARACTERS

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I. Complexity of Character and Difficulty of Learning [1]

The general purpose of this investigation was the determination of the relative difficulty of learning Chinese characters of varying degrees of complexity, when the number of strokes to a character was employed as the criterion of complexity.

The problem is one of considerable practical significance. Chinese characters, which have had a history of four thousand years, constitute the written language of more than four hundred million people. They are used by the Chinese, the Japanese, the Koreans and a part of the Indo-Chinese, embracing approximately one-fourth of the world's total population. Each of these characters is always an ideogram, and may contain from one to about thirty strokes. At present many Chinese educators are interested in the improvement of the written language. The agitation for reform is in two general directions; (1) simplification of the characters and (2) substitution of horizontal arrangement of the characters for the conventional vertical order. Some experimental data have been obtained in regard to the relative speed of reading Chinese in vertical and horizontal columns (3, 8); but hitherto there has apparently been nothing more than speculation concerning the basic problem of the nature or construction of Chinese characters. Moreover, the proper order of introduction of various types of characters into the reading and writing of the schools can be determined only through experimental investigations of the relative difficulty of these symbols.

In its theoretical aspect, the problem is related to the psychology of *Gestalt*. The theory of *Gestalt* originated in the field of perception, and has reference to the pattern characteristic of a given experience. The learning activity, according to it, is a total experience in that our reactions are to the object as a whole rather than to discrete elements. According to this theory, therefore, the perception of a Chinese character is a unitary affair, a Gestalt, and it is not to be explained as the result of the stimulation of the eye by a certain number of separate lines or strokes. The reaction is to the complete pattern of the character. Such a theory, if true, must stand the test of our investigation.

[1] This is the first of a series of researches on Chinese characters initiated and planned by L. S. Tsai, who prepared the manuscript. Miss Abernethy was responsible, with the kind assistance of Miss Helen Davis, for the collection of the data. The authors express their gratitude to Professors H. A. Carr, F. N. Freeman and C. H. Hamilton for their aid and interest in the work.

In the present study we have therefore attempted to undertake the experimental solution of the following specific problems:

(1) The relative difficulty of learning to recognize and to reproduce Chinese characters of a varying number of strokes.

(2) The relative difficulty of learning to recognize and to reproduce Chinese characters of an equal number of strokes.

(3) The reaction pattern of the subjects in learning to recognize and to reproduce Chinese characters.

(4) The course of progressive mastery in learning to recognize and to reproduce Chinese characters.

(5) The effect of practice in learning to recognize and to reproduce Chinese characters.

(6) The range of individual differences in learning to recognize and to reproduce Chinese characters.

Other Investigations Relating to the Six Problems

No investigations hitherto reported have a direct bearing upon the problems of this study; although the following types of experiment in perceptual and motor learning have a remote or indirect relationship.

(1) *Complexity of character and difficulty of learning*

(*a*) Length of Words.

With English words Bowden (1) found that, in general, the length of the word was not a signifiacnt factor in recognition. Gates and Boeker (6), however, concluded that "long words are harder to learn than short words, other things being equal."

(*b*) Length of Series.

The problem has been investigated by Ebbinghaus, Meumann, Binet and Henri, Henmon, Lakenan, Lyon, Strong, Kjerstad, Pyle, Pechstein, Robinson and Heron, Robinson and Darrow and others, in learning nonsense syllables, prose, poetry, numbers and maze problems. The general results are as follows: (i) difficulty increases with length, (ii) difficulty increases faster than length when difficulty is measured in terms of time, and (iii) difficulty increases less rapidly than length when difficulty is measured in terms of repetition. Since one repetition of a short series is not equal to one of a long series, time represents in this case the truer measure of difficulty.

(2) *Individual character and difficulty*

(*a*) Letters of alphabet. The study of the relative legibility of the letters of the alphabet was first investigated by Cattell and reported in Wundt's *Studien* of 1885 (2).

With the tachistoscopic method, Cattell was able to find that certain letters are relatively poor in legibility because of confusion, such as in the long narrow group, *f, j, i, l, t.* The suppression of the dot of the *i* was suggested, as well as the substitution of λ for *l.*

Later investigators have found somewhat different rank-orders of legibility for the letters, but all agree that some of the most commonly used letters are most easily confused. The letters *o, c* and *e* are usually found among the lowest in legibility. The letter *s* is hard to recognize. The letter *h* is confused with *b*, and the letter *a* with *u*. Notable studies of this problem are those of Sanford (10), Dockeray (4) and Roethlein (9). These studies have shown the need of reform in printing and the importance of differentiating more clearly certain confusable letters.

(*b*) Forms of word. Messmer divided words into two classes according to geometric form; 'linear' (*e.g.*, 'acorns' and 'saw') with no high letters, and 'superlinear' (like 'Eskimo'), words with high letters.

Two other classes have been added to those proposed by Messmer: 'sublinear,' such as 'going' with letters extending below the line, and 'superlinear and sublinear,' words with letters extending both above and below the line. Words were also classified by Messmer according to the type of line of which the letters of the word are made, *i.e.* as to whether the lines are straight, curved or oblique. Bowden's analysis (1) of the reading habits of six first-grade children indicates that there is probably little significance in such classifications. She found that "in only one case were the superlinear words, which Messmer holds to be more easily recognized, learned more readily than those of any other group." Gates and Boeker (6) have also reported that it is difficult to find "any type of word shape which is consistently easy or hard to learn."

(3) *Reaction patterns to characters*

(*a*) Reaction to whole. In Bowden's experiment, five children who had been learning to read for six weeks were given cards to read. The English words were first presented right-side-up, and five minutes later up-side-down. Three of the children "read with equal ease whether the card was right-side-up or up-side-down."

Bowden further experimented with shifting the letters of the word while preserving the general form, and found that these changes likewise caused little disturbance. Mutilations affecting the length or contour of the word gave, however, a considerable drop in the percentage of recognitions. It was concluded that the children "saw the words as wholes, and while not conscious of the position of the parts, were conscious of their presence or absence."

(*b*) Analysis of part. There is evidence that, while the general form of the word is important in recognition, the ability to perceive details is also an essential factor.

Bowden reports the case of a child who had trouble in distinguishing between 'sing' and 'song.' "When she had mastered the words, she was asked how she knew which was which. Her reply was 'by the looks.' When questioned further, she put her finger on the 'i' and the 'o.'" In an extensive investigation undertaken for the purpose of determining the nature of specific difficulties in reading and spelling, Gates (5) found that "the ability to perceive clearly the significant details of words" is a factor common to success in all perceptual tests involving words as materials.

(4) *Progressive mastery in learning characters*

Chinese writing is really drawing, to the Westerners, and it is thus related to a study of "the progressive mastery of certain simple percepts" by Judd and Cowling (7).

This investigation traces "the development shown by a number of subjects in the course of a series of tests in which they attempted to reproduce a simple figure which had been shown to them for ten seconds." The reproductions were made under varying conditions. From a comparison of the results, the following conclusions were derived; (i) the recognition of a figure in all of its details is a gradual process and (ii) in certain cases part of the figure may improve for a time and the same part may later be partially or completely overlooked.

(5) *Practice effect in learning characters*

The general problem of practice effect in learning has been incidentally studied by numerous investigators. The general results show that a few initial trials of practice have a very considerable effect in decreasing the difficulty of similar materials in subsequent learning.

(6) *Individual difference in learning characters*

Among the numerous incidental studies of individual differences in learning, the one most closely related to our problem is that by Judd and Cowling. They found that in the course of learning to reproduce figures there were individual differences in the rate of mastery of the size and relative position of the lines.

The Experiments

(1) *Learning to recognize Chinese characters.* The materials used in this experiment consisted of thirty paired associates. The first member of each pair was always a Chinese character, while the second member was an English word arbitrarily assigned as its equivalent in meaning. Three series of Chinese characters were used, with ten in a series.

In the first series, there were three strokes to a character; in the second, six strokes; and in the third, twelve strokes. Two sets of cards 3 x 5 inches were prepared. In one set, each Chinese character was inscribed upon a card together with its equivalent English word, which was typed in capitals. In the recall set, the Chinese character alone was inscribed upon the card. The Chinese characters were chosen at random from the Chinese dictionary. Each separate dot or line was counted as a stroke. The English words were selected from Thorndike's *Teacher's Word Book*. All the English words were monosyllabic. Precautions were taken to avoid words that were homonyms, that were easily spelled, or that might be associated in meaning with the form of the Chinese character.

Twenty-one graduate students served as subjects. They were all Americans. None of them had had any previous experience with Chinese characters. During the progress of the experiment, the subjects had no contact with Chinese characters except at the laboratory hours. Such advantages in control cannot be obtained with Chinese subjects or even with Westerners in China. Each of the twenty-one subjects learned all three series, and the order of learning was so arranged as to compensate for practice-effects.

Learning. Each pair in a series was exposed for 3 sec. Hence, 30 sec were required for a presentation of each entire series. Each series was learned in a day. Time was regulated by means of a metronome. No attempt was made to control the method of learning.

Recognition. Immediately following the first three presentations of each series there was inserted a test of recognition. Only the Chinese characters were given, and the subjects were allowed 4 sec for identifying each character and writing its English equivalent. After this first recognition-test, each presentation was followed by a similar test until the subject was able to recognize correctly all the characters in the series.

Retention. Retention was tested twenty-four hours after complete mastery of each series. An interval of one-half hour was inserted between this test of retention and the learning of any new series.

Reproduction. In order to investigate the reaction patterns to the characters, each subject was directed, after the last retention-test, to draw each character in the three series. He was told to draw as many lines of the character as he could remember, and, when the character was partially reproduced, to indicate by shading the general outline of the character.

At the conclusion of the experiment, each subject was requested to give an account of his method of learning to recognize the characters.

Three measures of difficulty were taken for each series; (1) the number of characters recognized at the first recognition-test following three presentations of the series, (2) the number of recognition-tests required to bring the entire series to the point of one correct and complete recognition and (3) the number of characters recognized at the retention-test one day after complete mastery.

(2) *Learning to reproduce Chinese characters*

This experiment was identical with the preceding except in the following points. Only nine subjects served in this experiment; in learning for reproduction the time of exposure was 5 sec (instead of 3) for each character in the three series and the time allowed for reproduction was 10 sec, as compared with 4 sec allowed for recognition. This longer period for reproduction was found necessary from preliminary

experimentation. The subjects were directed to try drawing as much of the character as possible during the five seconds of presentation. Reproduction takes the place of recognition in the tests inserted after the first three presentations, after each presentation thereafter, and twenty-four hours after complete mastery. In the reproduction test the English word was presented, whereas the Chinese character was presented in the recognition test of the previous experiment. Finally, in scoring the characters drawn, no attention was given to the size of the drawing. The criteria were the number of the strokes and the relation of the strokes within a character.

The Results

(1) *Complexity of character and difficulty*

The following Table (I) deals with the relations discovered to exist between complexity and difficulty of learning.

Table I

Complexity of character and difficulty of learning for *recognition*

Ser.	No. Subj.	Strokes in character	Recognitions req'd for mastery (av)	Characters recog'd after 1st 3 present'ns (av)	Characters recognized after 24 hrs (av)
I	21	3	3.71 ± .26	6.13 ± .33	7.38 ± .26
II	21	6	3.71 ± .35	6.38 ± .49	6.90 ± .30
III	21	12	3.48 ± .28	6.85 ± .21	7.59 ± .22

Complexity of character and difficulty of learning for *reproduction*

			Reproductions req'd for mastery (av)	Characters reproduced after 1st 3 present'ns (av)	Characters reproduced after 24 hrs (av)
I	9	3	3.66 ± .36	5.55 ± .48	9.33 ± .23
II	9	6	7.00 ± .39	2.00 ± .25	8.77 ± .48
III	9	12	9.66 ± .31	1.00 ± .20	8.33 ± .39

The results justify the following statements.

1. Difficulty of *learning for recognition* (whether immediate or delayed 24 hours) seems to be independent of the complexity of the characters within the limits of our investigation, *i.e.*, characters from 3 to 12 strokes.

2. Difficulty of *learning for reproduction* seems to increase directly with the complexity of the characters. In terms of the number of reproductions required for complete mastery, difficulty seems to increase directly with the logarithm of the number of strokes to a character.

In order to exemplify this relation, the following equations are proposed. They were suggested by Tsai and derived by F. O. Eppright.

Let

Y = the no. of reproductions required
X = the no. of strokes to a character
B, K, C = Constants and W, Z = variables

Y	K	Z	C	X	B	W
3.66 =	3 ×	1 +	.773	3 =	3 ×	1
7.00 =	3 ×	2 +	.773	6 =	3 ×	2
9.66 =	3 ×	3 +	.773	12 =	3 ×	4

When the Ys are plotted as ordinates against the Zs (left above), they fall approximately along a straight line,

$$Y = KZ + C. \tag{1}$$

By the method of least squares the values of the constants C and K are .773 and 3. respectively.

The number of strokes may be expressed as a function of a constant B and a variable W (right above).

It is obvious that X may be expressed as a function of B and Z as in the following equation:

By transforming equation (1) $X = B2^{z-1}$ (2)

and

$$Z = \frac{Y - C}{K} \tag{3}$$

Whence

$$Z - 1 = \frac{Y - C - K}{K} \tag{4}$$

Substituting this value in equation (2)

$$X = B2^{(Y-C-K)/K} \tag{5}$$

Taking the logarithm of both sides of equation (5)

$$\log X = \log B + \frac{Y - C - K}{K} \log 2 \tag{6}$$

Whence

$$Y = \frac{K}{\log 2} \log \frac{X}{B} + C + K \tag{7}$$

which for our data becomes

$$Y = \frac{3}{\log 2} \log \frac{X}{3} + 3.773 \tag{8}$$

3. Difficulty of *retention for reproduction* after one day's interval seems also to increase directly with the complexity of the characters. The differences, however, are very small though consistent.

4. No attempt was made to compare the relative difficulty in learning for recognition and for reproduction, since there were differences in the length of exposure as well as in the member of the pair presented at recall.

(2) *The various characters and difficulty*

The following Tables (II and III) express the difficulties in learning presented by the various individual characters.

TABLE II

RELATIVE DIFFICULTY OF INDIVIDUAL CHARACTERS FOR *recognition*

Character	Recogn'ns req'd for mastery	Rank order in learning	Times correctly given in retent'n test	Rank order in retention
		Series I		
工	28	1	18	3
川	30	2	19	1
丈	32	3	18	3
个	36	4	17	5
千	37	5·5	14	7·5
小	37	5·5	18	3
寸	42	7	14	7·5
土	50	8	14	7·5
上	55	9	14	7·5
大	59	10	9	10
		Series II		
州	30	1	16	4
竹	31	2	16	4
耳	33	3	15	6
亦	41	4	16	4
共	47	5·5	17	2
年	47	5·5	13	8
朱	50	7	13	8
在	51	8	19	1
米	53	9	7	10
羊	60	10	13	8
		Series III		
烏	23	1	21	1
豈	28	2	19	3
麥	34	3	20	2
甜	35	4	16	5
哥	37	5	18	4
羞	42	6	12	9
裁	44	7	13	8
黃	45	8	14	7
磨	55	9·5	11	10
婚	55	9·5	15	6

TABLE III

RELATIVE DIFFICULTY OF INDIVIDUAL CHARACTERS FOR *reproduction*

Character	Reprod'ns req'd for mastery	Rank order in learning	Times correctly given in retent'n test	Rank order in retention
		Series I		
千	13	1	7	10
大	14	2	8	7.5
个	15	3	9	3
工	17	4	9	3
川	18	6.5	9	3
小	18	6.5	8	7.5
土	18	6.5	8	7.5
寸	18	6.5	8	7.5
上	25	9	9	3
丈	26	10	9	3
		Series II		
竹	23	1	9	2
共	29	2	9	2
州	32	3.5	9	2
耳	32	3.5	8	6
米	33	5	8	6
在	41	6	8	6
羊	44	7	8	6
亦	45	8.5	8	6
朱	45	8.5	6	9.5
年	46	10	6	9.5
		Series III		
哥	28	1	9	2.5
鳥	29	2	9	2.5
麥	45	3	7	6.5
甜	48	4	9	2.5
豈	55	5	7	6.5
黃	59	6	8	5
將	60	7	9	2.5
裁	69	8	6	8.5
羞	75	9.5	6	8.5
唇	75	9.5	5	10

The above results may be summarized as follows:

1. Characters equal in number of strokes exhibit great differences in difficulty of learning. Within each series, the ratio of difficulty between the hardest and the easiest is approximately 2 to 1 in terms of the numbers of recalls required. This applies to both recognition and reproduction.

2. In *recognition*, not all of the most difficult characters are found in any one series, nor are all of the most easily learned found in another. They are rather distributed throughout all three series.

3. In *reproduction*, the most difficult characters are chiefly found in Ser. III, and the easiest in Ser. 1. Of ten characters learned with least number of recalls, eight are in Ser. 1; of ten learned with the greatest number of recalls, eight are in Ser. III.

4. Characters of equal number of strokes exhibit differences of difficulty in retention. The ratio is about 2 to 1 for recognition, and 3 to 2 for reproduction.

5. The rank orders of the characters in learning do not correspond exactly to those in retention. Nevertheless, all characters (with 2 exceptions in recognition and 4 in reproduction) which rank among the more easily learned halves of each of the three series, rank also among the better halves in retention.

6. The rank orders in learning for recognition do not correspond exactly to those in reproduction. But with only 2 exceptions all characters which stand in the more easily learned halves of each of the three series in recognition stand also in the better halves in reproduction.

(3) *Reaction patterns to the characters*

1. The following facts indicate that, in learning for *recognition*, the subject reacts to the particular pattern of the individual character as a whole rather than to the number of strokes of which the character is composed; characters of varying numbers of strokes are learned on the average with equal difficulty, and characters of equal number of strokes are learned with varying degrees of difficulty.

2. As to what constitutes the most easily learned patterns of the characters, no generalization can at present be made.

3. In *reproduction*, a certain part of the complex character stands out as a salient feature which the subject can reproduce first while he is still uncertain about the remainder of the figure.

4. As to what part of the character constitutes the salient feature, no uniformity has been discovered. Each subject seems to have his own reaction pattern to the characters.

(4) *Progressive mastery of the characters*

1. Progressive mastery is found only in learning to reproduce the more complex characters, and not those of three strokes.

2. The course of progressive mastery exhibits a combined process of analysis and synthesis. The subject first reproduces certain parts of the character, but not the remainder. When he gains mastery of the rest, he tends to lose what he first acquired. As practice continues, he regains what he first acquired, and assembles it into the complete character. This is in accord with the results of Judd and Cowling.

(5) *Practice effect in learning the characters*

TABLE IV

PRACTICE EFFECT IN LEARNING TO *recognize* THE CHARACTERS

Order Learned	Ser.	Recalls req'd	Ser.	Recalls req'd	Ser.	Recalls req'd	Total recalls for 3 series	Diff.	% of Total	Diff.
First.....	I	25	II	37	III	29	91		40	
Second.....	II	20	III	26	I	23	69	22	30	10
Last.....	III	18	I	30	II	21	69	0	30	0

PRACTICE EFFECT IN LEARNING TO *reproduce* THE CHARACTERS

First.....	I	17	II	21	III	28	66		36	
Second.....	II	26	III	31	I	9	66	0	36	0
Last.....	III	28	I	7	II	16	51	15	28	8

The following statements are derived from the data.

1. There is a practice effect in learning either to recognize or to reproduce Chinese characters.

2. The stage at which the practice effect appears varies with the modes of learning. Practice effect appears, as well as disappears, much earlier in learning for recognition than for reproduction.

(6) *Individual differences in learning Chinese characters*

TABLE V

INDIVIDUAL DIFFERENCES IN RELATION TO COMPLEXITY OF CHARACTERS

Ser.	Strokes	Sigmas for Recognition			Sigmas for Reproduction		
		Recalls req'd	No. correct after 3 presentns	No. correct after 24 hrs	Recalls req'd	No. correct after 3 presentns	No. correct after 24 h'rs
I.....	3	1.77	2.25	1.78	1.63	2.16	1.04
II.....	6	2.57	3.35	2.04	1.76	1.15	2.17
III.....	12	1.56	1.42	1.53	1.41	.93	1.76

INDIVIDUAL DIFFERENCES IN RELATION TO PRACTICE EFFECT (ALL 3 SERIES)

Order learned						
First.....	2.14	2.17	1.61	2.01	1.34	2.16
Second.....	1.54	2.14	1.78	3.16	2.98	1.52
Last.....	2.04	2.17	1.87	3.16	2.05	1.44

From the above results it appears that

1. There are individual differences in learning either to *recognize* or to *reproduce* Chinese characters.

2. There is no consistent variation in individual differences in accordance either with the increase of number of strokes to a character or with the amount of practice in learning.

SUMMARY

Our purpose has been to determine the relative difficulty of learning to recognize and to reproduce Chinese characters of varying degrees of complexity when the number of strokes to a character was employed as the criterion of complexity.

The present study is the first experimental approach to the problem of the nature or construction of Chinese characters, which have constituted the written language of a quarter of the world's total population for four thousand years.

Twenty-one American graduate students learned to recognize three series of Chinese characters of varying degrees of complexity, and another nine subjects learned to reproduce the same materials. Paired association of Chinese-English equivalents with inserted recalls was the method used.

The results indicate that *for recognition* the difficulty in learning is independent of the complexity of the characters within the limits used (characters of 3 to 12 strokes). *For reproduction* difficulty of learning seems to increase directly

with the complexity of the characters. In terms of the number of reproduction-tests required for complete mastery, difficulty seems to increase directly with the logarithm of the number of strokes to a character. These facts are based upon the means of each series of ten characters. Within each series, characters of equal numbers of strokes are learned with varying degrees of difficulty. *In recognition* the subjects react to the particular pattern of the character-as-a-whole rather than to the number of separate strokes. *In learning to reproduce* complex characters there is evidence that the subjects react first to some salient feature of the character and gradually to the rest during the course of progressive mastery. Practice effect and individual difference are found in both modes of learning which correspond to the reading and writing of Chinese characters.

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TRANSFER OF LEARNING

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It has long been known that practice of one part of the body in performing a skilled act increases the ability of the bilaterally symmetrical part in the same act. The effects of practice are transferred from one hand or one arm to the other, and from one toe to the similar toe of the other foot. This phenomenon has been called cross-education.[2] There are also indications that a similar transfer takes place to all parts of the body, not merely to bilaterally symmetrical parts. The experiments to be reported here were designed to study the transfer of learning from one limb to any other.

Bilateral transfer or cross-education was first noticed by Weber. In a letter to Fechner[3] he described the activities of his son, who was a spontaneous mirror writer, using his left hand in reversed fashion, although he had been taught to write only with the right hand. Weber concluded that in training the right hand, the left had been trained simultaneously to make symmetrical movements. Fechner reported a similar observation on himself. In the course of one of his investigations, he had frequent occasion to write the figure 9 with his left hand; then he found that when he used the right hand again he reversed the figure mirrorwise. A related phenomenon was discovered at the same time by Volkmann.[4] In an investigation on the two-point limen, he found that practicing the left arm in the discrimination of two points helped the right arm to make the discrimination. Further work showed that practice effects were transferred to symmetrical parts and to parts related in function but not to the rest of the body. These observations were later extended by numerous investigations into the subject. Scripture, Smith and Brown (pp. 114–119) devised an experiment to show transfer from right to left hand of the ability to thrust a needle into a small

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[1] This study was carried out at the Psychological Laboratory of Princeton University.

[2] E. W. Scripture, T. L. Smith and E. M. Brown, On the education of muscular control and power, *Stud. Yale Psychol. Lab.*, 1894, 2, 115.

[3] G. T. Fechner, Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite die der andern zugleich mitgeübt werden, *Ber. d. sächs. Gesell. d. Wiss., math. phys. Cl.*, 1858, 10, 70. (Reviewed by W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6.)

[4] A. W. Volkmann, Ueber den Einfluss der Uebung auf das Erkennen raümlicher Distanzen, *Ber. d. sächs. Gesell. d. Wiss., math. phys. Cl.*, 1858, 10, 38. (Reviewed by Davis, *op. cit.*)

hole without touching the sides of the hole. They first made a test of the left hand in this act and then practiced the right hand. A re-test of the ability of the left hand showed that it had improved 26 per cent. In the same way, they found that practice by the right hand in pressing a dynamometer resulted in a gain of strength of the left hand. Their method has been followed in numerous later experiments. It consists in making two tests of some part of the body and interposing a period of practice by some other part. The difference between the first and last tests is assumed to show the transferred improvement. Unfortunately the method fails to consider the effect of the first test upon the second. Acts of skill are known to improve greatly in the first few trials; so it is only to be expected that the second test will show an improvement over the first. For this reason the method described above gives no adequate measure of transfer. This defect was pointed out by P. H. Ewert,[5] who has shown that early estimates of the amount of transfer must be considerably discounted, and that a different method must be followed.

The defect in the method does not, however, completely invalidate the early work on transfer. Frequently it was found that the unpracticed part gained on its second trial as much as the practiced part gained in many trials. Thus in the work of Scripture, Smith and Brown it was shown that the effect of ten-days practice with the right hand caused an improvement in the left equal to the improvement shown in four days by the right. This seems fair evidence for the existence of transfer, although no measure of the amount can be so gained.

Following Scripture numerous experiments have been devised to show cross-education in skill, sensory education, fatigue and strength. In skill, Davis [6] found that practice of the right great toe in speed of tapping was shared to some extent by the left great toe and that accuracy in hitting at a mark with a fencer's foil was transferred from right to left arms. Woodworth [7] has shown that training in making accurate movements transfers from left hand to right when the training is successful. Unsuccessful training has no cross-effect. Other studies have shown cross-education in mirror drawing,[8] in juggling,[9] in ability to use a typewriter,[10] and, finally, in ability to set numbers in an adding machine.[11] In the field of sensory education several studies have been made of transfer. Dresslar [12] has repeated and confirmed the work of

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[5] P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 1926, 33, 235–249.

[6] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6–50.

[7] R. S. Woodworth, The accuracy of voluntary movement, *Psychol. Rev. Monog. Suppl.*, 1899, 3, (no. 13), 105 f.

[8] D. Starch, A demonstration of the trial and error method of learning, *Psychol. Bull.*, 1910, 7, 20–23; L. L. Henderson, The psychology of learning, (Bachelor's Thesis, 1906; deposited in the Library, Univ. of Wisconsin); H. S. Hill, Minor studies in learning and relearning, *J. Educ. Psychol.*, 1914, 5, 375–386; P. H. Ewert, *op. cit.*, 235–249.

[9] E. J. Swift, Studies in the psychology and physiology of learning, *Amer. J. Psychol.*, 1903, 14, 201–220.

[10] W. Frankfurther, Arbeitsversuche an der Schreibmaschine, *Psychol. Arb.*, 1914, 6, 419; A. Langelüddecke, Untersuchungen über die Vorgänge beim Erlernen des Maschinesschreibens unter normalen und krankhaften Bedingungen, *Psychol. Arb.*, 1923–25, 8, 304–414.

[11] W. H. Norcross, Experiments on the transfer of training, *J. Comp. Psychol.*, 1921, 1, 317–364.

[12] F. B. Dresslar, Studies in touch, *Amer. J. Psychol.*, 1894, 6, 324–330.

Volkmann on transfer in tactual discrimination. Experiments on the conditioned reflex show a similar phenomenon. Anrep [13] has found that scratch stimuli on either side of the body are equivalent in calling out salivary secretion; although only one side of the body has been used in establishing the conditioned reflex. Cross-effects in sensory processes do not always occur, however, for one author [14] finds that adaptation to weights is not transferred from one hand to the other.

Experiments on the transfer of fatigue give somewhat ambiguous results, for cross-effects can be both positive and negative. Bryan,[15] by testing the rate of tapping from various joints, found crossed fatigue effects, independent of any general bodily fatigue. Peculiarly enough, however, one subject showed an opposite cross effect, that is, the fatigue of one joint caused an increase in the rate of the same joint of the other side. This result is confirmed by Joteyko,[16] who likewise finds positive and negative cross-effects in fatigue. The nature of the transfer seems to be constant in any one individual.

Transfer of strength has been shown by Davis who found that the left arm gained in girth, strength (as measured by a dynamometer) and endurance by exercise of the right arm in swinging a dumbbell, pressing a dynamometer or pulling an ergograph.[17] Similar results are shown by Wissler and Richardson.[18]

The defect in method as noted above has been remedied by Ewert in mirror drawing. Ewert used two groups of subjects, one of which followed the usual procedure. The other group acted as controls, receiving only the end-tests of the unpracticed part. Comparison of the relative improvement of the two groups gives an adequate measure of transfer. Ewert found that the non-preferred hand improved 36 per cent. in time and 21 per cent. in errors by fifty-trials practice with the preferred hand. Measured in the old way the transfer would have been estimated at 82 per cent. in time and 76 per cent. in errors. This shows that previous estimates of transfer have been too high, but that some transfer actually occurs. Ewert was also able to show that transfer is greater from the preferred to the non-preferred hand than in the opposite direction.[19]

There are some indications that transfer takes place to parts other than those bilaterally symmetrical. As mentioned above, in the two-point limen Volkmann found that parts related in function shared in the practice of one finger; *i.e.*, other fingers shared in the practice but the arm did not. Other investigators have obtained similar results. Davis [20] found that practice in speed of tapping the right great toe was shared by the index fingers of both hands. The left index finger gained almost as

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[13] G. V. Anrep, Irradiation of conditioned reflexes, *Proc. Roy. Soc. Lond.*, 1922–23, 94B, 409.

[14] L. Steffens, Ueber die motorische Einstellung, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 1900, 23, 241–265.

[15] W. L. Bryan, Voluntary motor ability, *Amer. J. Psychol.*, 1892–93, 5, 149.

[16] J. Joteyko, Participation des centres nerveux dans les phénomènes de fatigue musculaire, *Année psychol.*, 1900, 7, 161–165.

[17] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 18–29; 1900, 8, 64–109.

[18] C. Wissler & W. W. Richardson, Diffusion of the motor impulse, *Psychol. Rev.*, 1900, 7, 34.

[19] *Ped. Sem.*, 1926, 33, 235–249.

[20] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6–50.

much as the part practiced while the right showed a slightly smaller improvement. The validity of this distinction between fingers is questionable, for Davis had no control-subjects and the difference was small.

Lashley [21] also noted transfer-effects in the course of work on cerebral function in learning. He taught monkeys to open a problem box with the right hand. He then destroyed the motor area of the left cortex, thus paralyzing the right hand. When the animals were again placed in front of the problem box, they used the left hand, the feet and sometimes the head to open it. The animals were said to show little or no random movement in using these previously unpracticed parts. In one case transfer to the left arm occurred although it had been temporarily paralyzed during learning by the right.

Theory of Transfer. Several types of explanation have been proposed to account for transfer from one part of the body to others. The early investigators found an explanation in terms of the improvement of some faculty.

Davis, for example, concluded that training the attention and will-power explained in part the transfer of ability to tap rapidly (p. 49). Scripture [22] finds that training the will-power explains crossed gains in strength. Explanation by faculties is no longer acceptable in psychology, for it is a purely verbal explanation. Furthermore, investigations of the doctrine of formal discipline militate against the faculty explanation.

Other explanations of cross-education and transfer from one part of the body to others have been drawn from the related field of transfer from one situation to another. The voluminous work on this subject has led to two theories, that of *identical elements* and that of *generalization*, both of which seem applicable to the type of transfer considered here. The theory of identical elements would say that transfer from one part of the body to another takes place where the subject learns part-activities involved in the performances of both members. Thus Woodworth [23] suggests that common head, eye or trunk movements may be involved in the performance of an act of skill or strength by either hand.

The theory of generalization does not differ greatly from that of identical elements except that it emphasizes conscious rather than behavioral factors. It suggests that transfer is due to the generalization of methods, attitudes or ideals learned in practice by one part of the body and applied to the performance of the act by other parts. This type of explanation is used by Swift [24] in his work on transfer of ability to juggle two balls. He found that methods of handling the balls, of throwing them, and of recovering from bad throws were developed by one hand and then carried over to the other.

The two theories are more or less complementary. They can therefore be combined and called the theory of *common elements*.[25] There seems to have been only

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[21] K. S. Lashley, Studies of cerebral function in learning, *Arch. Neur. & Psychiat.*, 1924, 12, 249–276.

[22] E. W. Scripture, Recent investigations at the Yale Laboratory, *Psychol. Rev.*, 1899, 6, 165.

[23] R. S. Woodworth, *Le mouvement*, Paris, 1903, 325.

[24] E. J. Swift, Studies in the psychology and physiology of learning, *Amer. J. Psychol.*, 1903, 14, 201–220.

[25] J. V. Breitwieser, *Psychological education*, N. Y., 1926, 164.

one effort to isolate experimentally common elements of method or behavior and to determine to what extent they play a part in transfer. Norcross, in his work on bilateral transfer of ability to place numbers in an adding machine, tried to isolate common elements, such as ability to read quickly and accurately the numbers to be listed, familiarity with the keyboard, actual use of the keyboard apart from the setting of the experiment, etc. He found that practice with any of these part processes alone acted favorably on ability to perform the act as a whole. Unfortunately Norcross [26] did not use many subjects. As his data show a large individual variability, his results are not necessarily conclusive evidence for the theory of identical elements. It is further to be regretted that he used no control subjects, for the gains reported (from 7% to 48%) might easily be due to practice rather than to transfer.

One further possibility has been advanced to explain transfer. Davis suggests (p. 49) that there may be some actual training of the so-called unpracticed part. This might take place, he suggests, by a spread of the nervous impulse to the motor centers governing the unused part, or by spread of the impulse to the muscles themselves, so that the apparently unused part might actually receive practice. The same possibility is advanced by Woodworth, who says, "L'exercise d'une main n'est pas entièrement séparé de celui de l'autre; les mains sont souvent innervées ensemble et donc entraînées spécifiquement en meme temps" (p. 325).

There is a wide variety of evidence showing the close relationship between the parts of the body, especially between the two sides. It is known, for example, that injuries to the left side of the cortex disturb skilled acts performed by either side of the body.[27] And in cases of hemiplegia a paralyzed arm may be made to move by movement of the normal arm. These movements are frequently symmetrical to those of the normal arm.[28] Observations of this sort show the close relationship between different parts of the body. There has been no evidence advanced, however, to show that associated movements of the so-called unused part are of any importance in transfer.

To summarize:—There are current two theories of transfer; the theory of common elements and the theory which assumes some sort of practice of the apparently unused member. It is one of the aims of this investigation to furnish experimental data, which may throw light on the relative validity of these two theories.

The Experiments

In order to study transfer from one part of the body to others, mirror drawing was chosen as a task. The procedure for the hands followed that described by Dearborn [29] and used by Starch and Ewert. The subject traces the outline

[26] W. H. Norcross, *op. cit.*

[27] H. Liepmann, Die linke Hemisphäre und das Handeln, *Münch. med. Woch.*, 1905, 48. (Reviewed in *J. f. Psychol. u. Neur.*, 1906, 7, 190.)

[28] F. M. R. Walsche, On certain tonic or postural reflexes in hemiplegia with special reference to the so-called associated movements, *Brain*, 1923, 46, 136.

[29] W. F. Dearborn, Experiments in learning, *J. Educ. Psychol.*, 1910, 1, 374–378.

of a star which he can see only through a mirror. An effort was made so to adapt the experiment that the task could be performed by the feet as well as by the hands. The technical difficulties as well as the difficulty of the task prevented the attempt to obtain transfer from hands to feet; but the impression of the experimenter and that of the subjects as well was that the feet gained considerably from the practice of the hands. Transfer from right to left hand was shown by the results; but because of certain errors in the procedure this work was unsatisfactory. The results confirmed, in general, those of Ewert on transfer from preferred to non-preferred hand.

It seemed to be desirable to obtain a method by which transfer from hand to foot could be adequately studied. For this purpose a new experiment was undertaken which involved a combination of two old experiments on the acquisition of skill, namely a mirror-experiment and an accuracy-experiment. The task set the subjects was to hit at a target seen indirectly through a mirror.

The subject sat at a table placed against a wall. Mounted on the wall was the target, consisting of a cross drawn on a sheet of white paper, 8 x 11 inches in size. Direct view of the target was prevented by a wooden screen, set above the table and directly in front of the subject. At the subject's left was a mirror placed at right angles to the wall. By means of the mirror an indirect view of the target could be obtained. The subject, holding a pencil in his hand, placed his arm under the screen with his elbow resting on a spot marked on the table. This spot was directly in front of the center of the cross. The arm was held upright and in such a position that the back of the pencil touched the screen. The subject, looking into the mirror, hit at the target, with the purpose of coming as close to the center of the cross as possible. After hitting he brought his arm back to the starting position, so that the pencil again touched the screen. Then he hit again at the target, trying to correct any previous error. The arm was again brought back to the starting position and the procedure repeated. The speed of hitting was controlled by a metronome beating at 72 per minute. On one beat the subject hit forward, on the next he returned to the starting position, and so on. By having the subject hit in time to the metronome and by having him return to the screen after every hit, both the speed and the amplitude of hitting were controlled. After ten successive hits the subject was told to stop and was given a rest period of two minutes. A new target was then placed on the wall and the procedure repeated.

For practice with the foot a similar procedure was used. The subject sat in an elevated chair with his thigh in a rest. This rest was directly in front of the target, so that the foot hung in front of the mark. The target was obscured by a screen, but on the left was a mirror in which it could be seen indirectly. A pencil was securely

attached to a light block of wood, which was strapped firmly to the subject's foot. By this means the subject could hit at the target and leave a mark on it. Speed and amplitude of hitting were controlled in the same manner as above. Typewritten instructions were given to the subjects.

> The object of this experiment is to hit several times in succession at the center of the cross you see in the mirror. The hitting is to be done in time to the metronome, which beats one stroke for the forward movement and one for the backward. After the forward movement return always until you hit the board in back of your hand (foot). In hitting try not to hesitate but hit with one single jab.
>
> After a certain number of trials you will be told to stop and there will be a short intermission. During each of these intermissions please give a report of your experience during the test, noting methods, if any are used, confusion due to the necessity for timing the hits, or anything important to the experiment. Try not to let this report interfere with your attention to the experiment, however. Make it retrospective. Remember particularly these points:—
>
> Each hit is to be a single jab at the mark.
>
> Keep in time to the metronome.
>
> Hit the board in back of your hand on the backstroke.

The experimenter had at his disposal the sheets of paper on each of which were ten pencil marks representing the subject's efforts for that trial. Each set of ten hits was called a trial. The average distance of the ten hits from the center of the cross gave a measure of the subject's accuracy on that trial. This distance was obtained by laying the sheets of paper over a sheet of graph paper, ruled in 2-mm squares and illuminated from below. The distance of each hit could be determined by counting the number of squares between it and the target, in the horizontal direction. By means of the graph paper a convenient and accurate method of measuring the performance of the subject was obtained.

It was decided to eliminate the preliminary test of the unpracticed member, as a test of this sort makes it impossible to determine the effect of transferred practice on the first trial with the unpracticed part. It was furthermore desirable to determine the effect of transferred practice on later trials with the new part. Consequently a measure of transfer was obtained by using two groups of subjects, one of which received practice with one part of the body, followed by practice with another part, while the other group received only practice with the latter. In the preliminary experiment undertaken here, one group of four subjects received 60 trials on the right hand followed by 40 trials with the right foot. The other group of four subjects received 40 trials with the right foot. Comparing the records of the two groups on the foot gave a measure of the effect of the hand-practice on the performance of the foot.

A considerable difference between the two groups was found. The effect of the previous practice with the hand was to decrease considerably the amount of the initial error of the transfer subjects. At the beginning of practice there was a large difference between the two groups. This difference decreased considerably with successive practice. Because of the small size of the two groups no definite conclusions could be drawn; but it was felt that an adequate method of studying transfer had been found. For further work it was thought best, however, to simplify the experiment by making the target a simple vertical line instead of a cross. The mirror reverses in the horizontal direction only and to study the transfer of a mirror coördination it is best to limit the practice to that form of coördination alone.

The target, then, was changed to a simple vertical line, bisecting the sheet of paper. The instructions were changed to meet this new condition. They also emphasized more strongly the necessity for accuracy and cautioned the subjects against practicing between experimental periods or talking about the experiment with other subjects.

Transfer from Hand to Foot

Two new groups of subjects were obtained for the changed experimental conditions. Group I (14 Ss) was given sixty trials

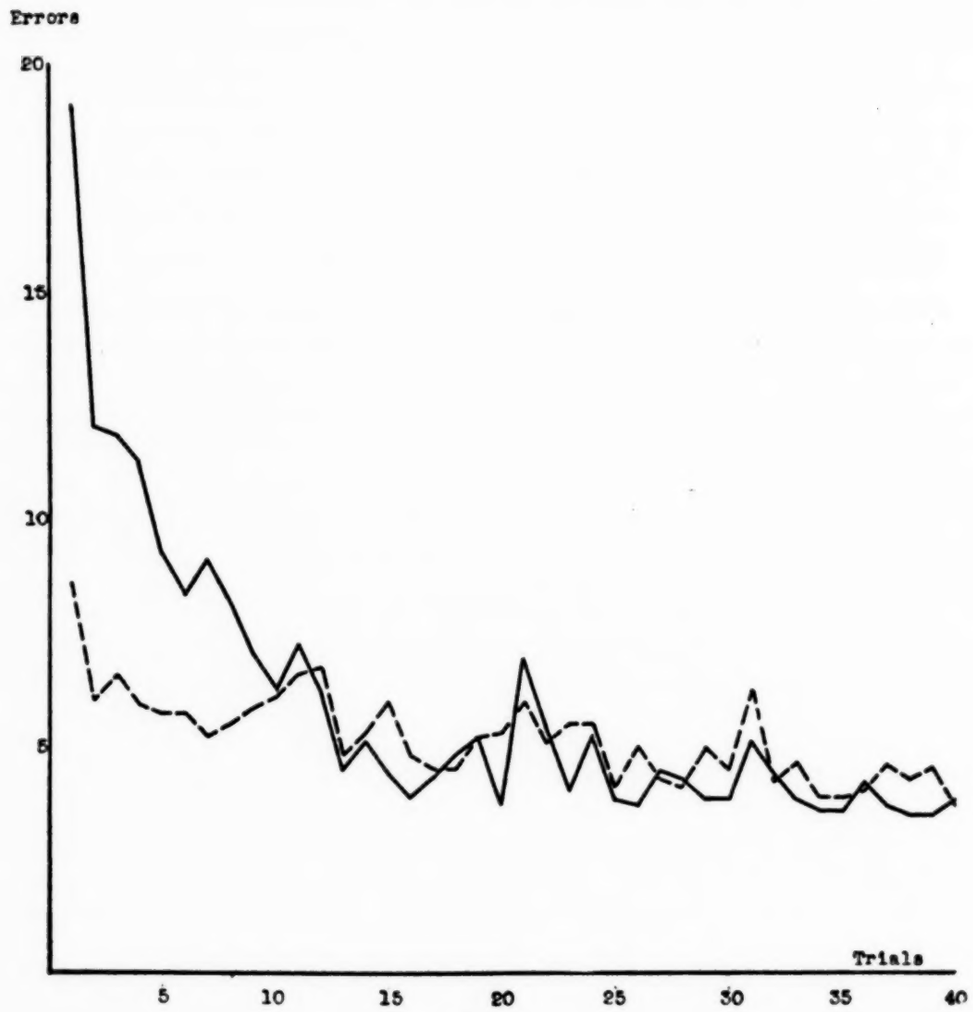

FIG. 1
— — — Group 1
——— Group 2

practice with the right hand followed by forty trials practice with the right foot. Ten trials were given in one sitting and with a few exceptions sittings were given on successive days.

Group II (14 Ss) received forty trials with the right foot. It should be mentioned that in these, as in all groups to follow, only right-handed subjects were used.

The average foot-records of the two groups are compared in Figure 1. They reveal a typical learning curve for Group II, in this case the control group. There is a high initial error, which decreases fairly rapidly in the first few trials, but with slow improvement after the first ten trials. Group I, the transfer group, shows a very different curve; a small initial error, a large fall from the first to the second trials and a period of slow improvement thereafter. The curves of the two groups meet and remain the same after the tenth trial. The initial error of the transfer group is approximately equivalent to that made by the control group on the sixth trial. The transfer group, therefore, has saved the equivalent of about six trials by reason of its previous practice with the hand.

The differences between the two groups are statistically significant. It was found that the difference on the first trial was over seven times its P.E. For the first four trials the differences are more than four times their probable errors, indicating that the differences on these trials are not due to chance. After the fourth trial the differences are less than four times their probable errors, the statistical significance decreasing with the size of the difference.

Group I, the transfer group, had received a large amount of practice on the hand. In order to ascertain the effect of decreasing the amount of hand-practice, two new groups were obtained. Group III (11 Ss) was given 20 trials practice with the right hand, followed by practice with the foot. Group IV (12 Ss) 10 trials practice with the hand, followed by the foot. Comparing the averages of these two groups on the foot with that of Group I shows the effect of 60, 20, and 10 trials practice with the hand on the subsequent practice with the foot. The averages for the three groups are shown in Figure 2.

There are practically no differences between the three groups. With the exception of a few small variations the

three curves are the same throughout.[30] Varying the amount of hand-practice from ten to sixty trials has no effect on transfer of learning to the foot. This means that the slight improvement characteristic of the hand-practice after the first

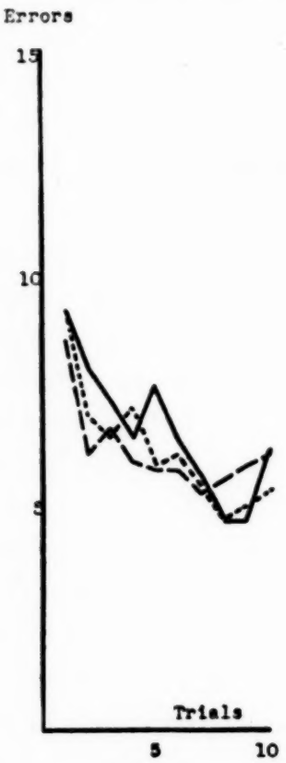

FIG. 2

——— Group 1
——— Group 3
- - - - - Group 4

ten trials is not transferred, or at least, that it has no observable effect in this experiment.

A study of the individual curves of Groups III and IV reveals an interesting and important fact. In these groups the majority of the Ss show very little if any improvement on the foot after the first two or three trials. They show a small

[30] This statement holds true only after one subject of Group III has been thrown out on the second and third trials. On these two trials this S was extremely poor, but he returned to about average in a few trials. On the second and third, he seemed to suffer a breakdown, lost all control over himself and appeared very embarrassed and self-conscious. Inclusion of his record on these two trials would distort the results of the rest of the group.

initial error and very little, if any, improvement. That is, they have almost nothing to learn. A few Ss, however, (a total of five in the two groups) show a large initial error and improve relatively slowly. Their curves are quite like the curve for Group II, the control group. Examination of the hand-records of these individuals shows that in every case either no improvement had been made on the hand or that the stage of slow improvement had not been reached when the hand-practice was discontinued. In all other Ss of these two groups the first drop in the learning curve of the hand had been completed and the secondary stage of slow improvement had been started when hand-practice was discontinued. For these latter Ss, curves for the foot typically like that for the average of the transfer group were obtained. It would seem that unless those adjustments which cause the first drop in the learning curve of the hand have been completed, no transfer takes place. Furthermore, these results show that the slow, gradual improvement, characteristic of the later periods of hand learning, has no observable effect on the foot.

Transfer from Foot to Hand

In order to study transfer from foot to hand it was only necessary to give Group II practice with the right hand following its practice with the foot. Group II, therefore, was given 40 trials with the right hand following practice with the right foot. Comparison of its record on the hand with that of Group I gives the effect of previous practice with the foot upon the performance of the hand.

The averages of Group I and II on the right hand are shown in Figure 3. There is a slight difference in favor of Group II, which is, in this case, the transfer group. Although the difference is not great it is quite constant and lasts throughout the practice. The differences are not significantly greater than their probable errors. Consequently the question is raised whether the difference between the two groups is the result of transferred learning or whether it is not the result of a chance difference in ability between the two groups. In the work on transfer from hand to foot, trans-

fer was only effective in the first part of learning. Likewise experiments on transfer from one situation to another have shown that transfer is effective only in the first stages of learning. When a rat, *e.g.*, has learned to run one maze, the

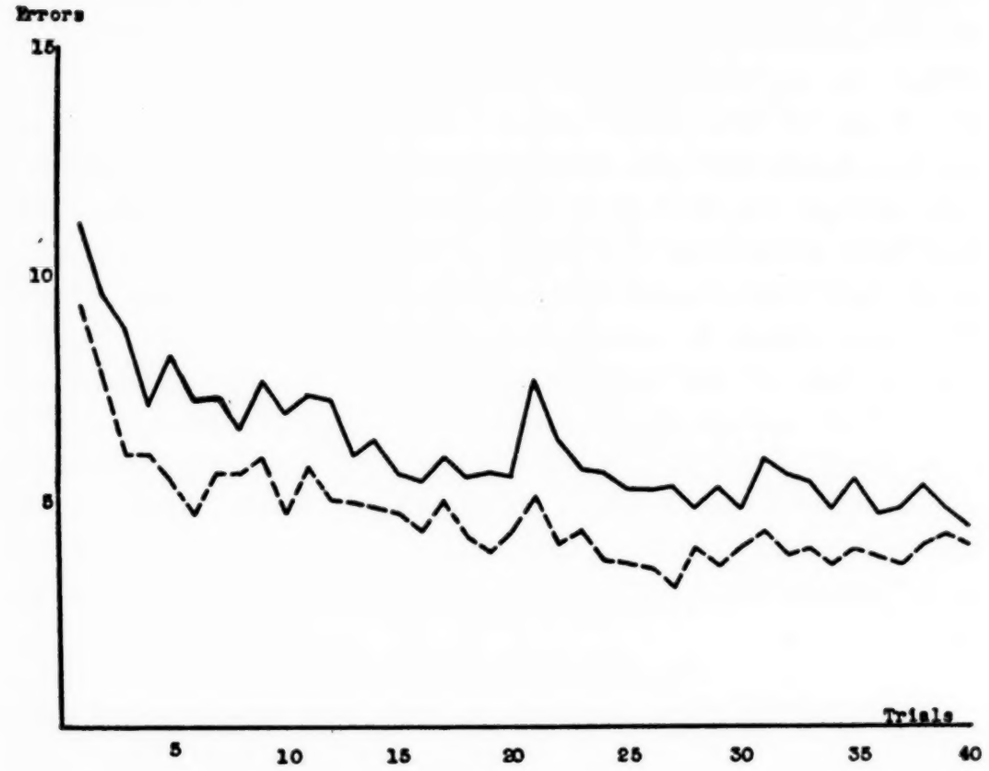

FIG. 3

——— Group 1
— — — Group 2

training affects the first stages but not the later stages of learning a subsequent maze.[31] These facts would indicate that the results of the present experiment are due to chance rather than to transfer.

On the other hand, there is a possibility that transfer from foot to hand occurs but is obscured by a certain factor. During the hand-practice it was observed that the hand frequently changed position with respect to the arm while the subjects were hitting. In the great majority of subjects the change bore no relation to corrections but appeared to be a

[31] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.*, 1917, 24, (no. 104).

chance affair. The phenomenon was occasionally reported by subjects as a source of error that they found impossible to control. The hand could not be kept in a constant position with relation to the arm. This introduced chance errors into the experiment, and it was thought that a chance error of this sort might serve to mask transfer effects. Accordingly a new experiment was devised in an attempt to rule out this factor.

The pencil was attached to a light piece of wood ten inches in length. On one end of the device a rounded block of wood was placed, which could be grasped comfortably by the hand. When held in the hand, the strip of wood extended nearly to the elbow, being strapped to the arm in two places. By means of this device, the wrist and hand were prevented from moving except in connection with movements of the whole arm, and the above mentioned source of error was eliminated.

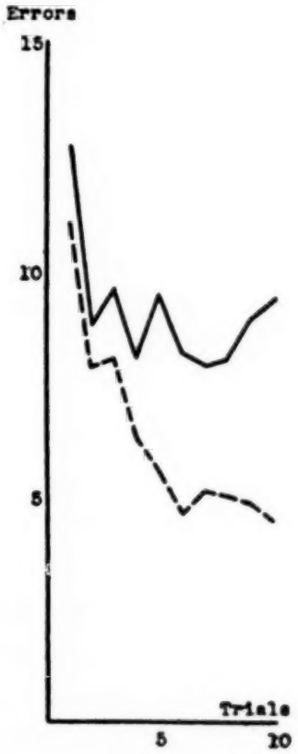

FIG. 4

——— Group 5

——— Group 6

Two new groups of subjects were obtained. Group V (10 Ss) received ten trials with the right foot followed by ten trials with the right hand. Group VI (11 Ss) received ten trials with the right hand. The averages of the two

groups are compared in Figure 4. Throughout the practice there is again a slight difference in favor of the transfer group. On the last three trials the difference between the two groups increases due to the fact that three out of the eleven Ss in Group VI showed a marked increase in errors on these trials. In the case of two of these Ss it is believed that the increase in error is due to a loss of interest. Reports obtained from them after the experiment confirm this hypothesis. The result of this further study of transfer from foot to hand supports the conclusion that practice with the foot has some slight effect on subsequent practice with the hand, although the effect seems to be less than in the case of transfer from hand to foot.

Transfer from Hand to Hand

In order to study transfer from one hand to the other, Group VI, which had already received ten trials practice with the right hand, using the device for controlling hand and wrist movements, was given ten trials with the left hand, using this same device. Another group (VII) was given ten trials with the left hand followed by ten with the right. With these two groups transfer from right to left hand and from left to right could be studied.

In this experiment a new condition was introduced. When the subjects changed from one hand to the other the position of the mirror was changed. When the right hand was used, the mirror was in its normal position at the subject's left, as in the previous experiments. In using the left hand, however, the mirror was placed in a similar position but to the right of the subject. The change probably has little or no effect on the experiment, for it merely causes a shift in absolute cues, such as nearness to, and distance from, the mirror of objects near the target. On the other hand, the posture of the subject is considerably changed, for he must hold his neck in opposite positions in the two cases. It is not believed that this change affected the results.

In Fig. 5 the averages of the two new groups for the left hand are shown. The graph shows that there is a slightly

greater inaccuracy for the left hand as contrasted with the results already obtained for the right. But again the general fact of transfer stands out. For Group VI, the transfer group is slightly better than VII, the controls.

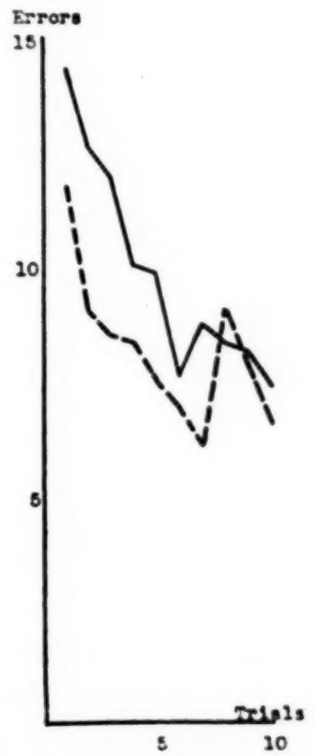

FIG. 5
— — — Group 6
——— Group 7

Quite similar results are found in Fig. 6 where the curves of the two groups for the right hand are shown. These curves are not so smooth and regular as those previously shown; but Group VII, which in this case is the transfer group, presents a lower curve than VI.

In all the experiments reported here on transfer from one hand to the other, the differences between control and transfer subjects are small, and there is considerable overlapping between the control and transfer groups. Nevertheless, the fact that every group which has received previous practice with some other limb makes less error than its corresponding control-group is strong evidence for the existence of transfer.

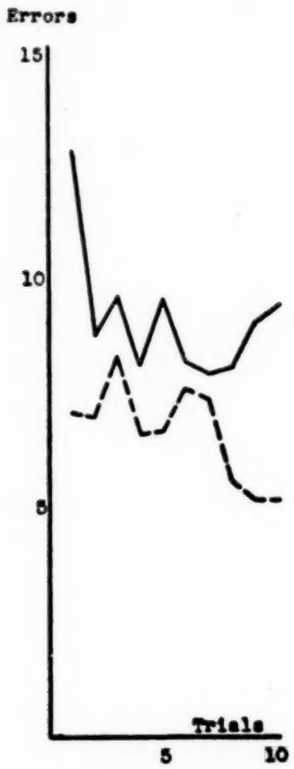

FIG. 6

——— Group 6
— — — Group 7

The results, shown graphically above, are summarized numerically in Table I. The average of each group, together

TABLE I

RIGHT FOOT

	Control Subjects					Transfer Subjects						
	Group 2		Group 5			Group 1		Group 3		Group 4		
Trial	Av.	M.v.	Av.	M.v.	Av.	Av.	M.v.	Av.	M.v.	Av.	M.v.	Av.
1....	19.1	5.9	20.5	8.8	19.7	8.6	2.9	9.2	4.3	9.2	6.0	9.0
2....	12.0	3.4	13.9	7.4	12.8	6.0	2.6	7.9	3.0	6.9	3.6	7.8
3....	11.8	5.4	10.9	3.7	11.4	6.6	2.2	7.3	1.5	6.4	3.2	8.0
4....	11.3	3.3	8.1	3.2	9.9	5.9	2.1	6.4	2.7	7.1	3.4	6.5
5....	9.2	5.6	8.3	2.9	8.8	5.7	2.5	7.6	3.7	5.8	2.9	6.3
6....	8.3	4.3	7.0	2.9	7.8	5.7	2.4	6.4	3.3	6.1	2.4	5.9
7....	9.1	4.4	5.7	1.5	7.7	5.2	2.3	5.6	2.2	5.4	2.3	5.4
8....	8.2	2.8	6.3	2.1	7.4	5.5	2.4	4.6	1.4	4.7	2.0	5.0
9....	7.0	2.2	5.6	1.7	6.4	5.8	2.1	4.6	1.8	4.9	1.8	5.1
10....	6.2	2.0	5.6	2.1	5.9	6.1	3.0	6.2	2.2	5.3	1.6	5.5

RIGHT HAND

	Control Subjects						Transfer		Control		Transfer			
	Group 1		Group 3		Group 4			Group 2	Group 6		Group 5		Group 7	
Trial	Av.	M.v.	Av.	M.v.	Av.	M.v.	Av.	Av.	M.v.	Av.	M.v.	Av.	M.v.	Av.	M.v.
1..	11.1	2.5	11.4	4.3	13.7	5.6	12.1	9.3	3.0	12.7	3.9	11.0	4.4	7.0	2.3
2..	9.5	2.5	9.6	3.7	9.9	4.4	9.7	7.7	2.4	8.7	3.9	7.8	3.2	6.9	2.2
3..	8.8	4.2	8.9	4.2	9.0	4.4	8.9	6.0	1.4	9.5	3.1	8.0	2.9	8.2	3.8
4..	7.1	2.0	7.8	2.6	8.8	3.8	7.9	6.0	1.9	8.0	3.3	6.2	1.8	6.5	2.4
5..	8.2	1.8	7.5	2.9	7.6	3.1	7.8	5.5	1.3	9.4	4.1	5.5	1.3	6.6	3.0
6..	7.2	2.2	8.3	4.4	7.0	2.1	7.5	4.7	1.4	8.1	4.0	4.6	1.4	7.5	5.1
7..	7.3	2.4	6.8	3.2	8.6	3.3	7.6	5.6	2.2	7.8	2.6	5.1	1.0	7.3	2.8
8..	6.6	2.5	5.1	1.5	6.1	2.6	5.9	5.6	2.2	8.0	4.3	5.0	1.8	5.5	2.7
9..	7.6	2.2	5.7	1.9	5.7	1.9	6.3	5.9	2.8	8.8	4.7	4.8	1.6	5.1	2.5
10..	6.9	2.9	5.6	1.8	7.1	2.4	6.5	4.7	1.8	9.3	5.4	4.4	1.1	5.1	1.7

LEFT HAND

	Control Subjects		Transfer Subjects	
	Group 7		Group 6	
Trial	Av.	M.v.	Av.	M.v.
1	14.3	4.2	11.7	3.4
2	12.6	4.8	9.0	2.9
3	11.9	4.5	8.4	2.4
4	10.0	2.3	8.3	2.0
5	9.8	2.8	7.5	2.4
6	7.6	1.9	6.9	1.3
7	8.7	2.9	6.1	1.2
8	8.3	3.0	9.0	2.8
9	8.1	3.0	7.7	2.3
10	7.3	1.7	6.5	2.6

with its mean variation, is shown for the first ten trials with each limb. In a few cases the average of several groups taken as a single unit is also given. Thus Groups II and V, both of which commenced with practice of the right foot, are combined into one average. Likewise Groups I, III and IV are combined for the right hand and foot. An examination of this table shows, as did the graphs, that in every case studied some transfer has been found. In order to facilitate comparison of the extent of transfer with practice by the various limbs, the percentage of error saved by each transfer-group in comparison with its corresponding control-group was calculated for the first ten trials. This calculation brings out merely the facts already noted that transfer is more effective from right hand to right foot than in any other case. The saving of groups transferred to the foot is reduced, however, to zero in the course of the first ten trials; whereas in the other cases the saving appears to last for an appreciably longer time, and in the one case where it has been studied it lasts for forty trials. No other differences in extent of transfer

can be obtained, as the averages are probably not sufficiently stable to admit accurate comparison of the various groups.

Discussion of Results

To explain transfer it is necessary to analyze the behavior of the subjects and to note the elements of practice that were common under the various conditions.

There were several factors which seemed to have no bearing on transfer and which may be readily disposed of. Thus the necessity for timing the hits in accord with the metronome and the necessity for returning to the starting position after every hit affected the accuracy of hitting. At the beginning of practice many subjects found it impossible to maintain the proper rhythm unless they attended closely to the speed of hitting. A few subjects likewise reported that the necessity to return to the starting position after every hit distracted their attention from accuracy and the reversing effect of the mirror. It has been shown by Schwarz [22] that in substituting a new habit for an old, a loss of attention causes the subject to revert to the old. In the present case the reversion would be to the normal habits of space-coördination and would result in inaccuracy of hitting. It is not believed, however, that this factor was of importance in transfer, for no evidence of improved rhythm or better adjustment to amplitude of swing was found in transferred subjects.

One other factor of importance in learning but of little or no importance in transfer was the position of the body with respect to the arm or leg. The bodily position was not controlled in the experiments, the subjects taking a position comfortable to them. Usually the position remained constant throughout any one sitting but sometimes changes occurred. The effect of change seemed to be temporarily to increase error unless the change brought the upper arm and shoulder into line with the target. In most cases shifting the body increased error and thus was probably one factor in causing variability. This factor had no apparent bearing on transfer, however.

Of more importance to the study of transfer were certain other factors. Chief among these was adjustment to the reversing effect of the mirror. At the beginning of practice the first hit was largely a matter of chance. The second, however, almost invariably carried the subject further away from the target because of the mirror-effect. The third hit might carry the subject still further away. Finally, a movement in the proper direction would be made. This was usually accomplished with a great deal of effort and in the hand-practice was frequently accompanied by movements of the whole body. A movement in the right direction was not,

[22] Untersuchungen zur Handlungs- und Affektspsychologie; iv. Ueber Rückfälligkeit bei Umgewöhnung; i Tl. Rückfalltendenz und Verwecklungsgefahr, Georg Schwarz, *Psychol. Forsch.* 1927, 9, 105.

of course, always successful in bringing the subject close to the target. Consequently a further correction was needed which might result in still another error of direction. Errors of direction dropped out rapidly in the course of the first few trials and occurred but rarely at the end of the first sitting.

Next to the difficulty in adjusting to the reversing effect of the mirror, the greatest difficulty was to correct by the proper amount. There was an almost universal tendency to overcorrect; *i.e.*, in correcting a previous error there was a tendency to go beyond the target to the other side. This source of inaccuracy was also eliminated almost completely in the course of the first sitting.

It is believed that these two factors are the most important in the experiment. An effort was made to observe whether errors of direction and overcorrections occurred more frequently in control than in transfer groups. To this end the experimenter counted the number of times that either appeared to occur in any trial. The accuracy of counting in this way is undoubtedly low; but it is significant that all transfer groups showed fewer errors of direction and fewer overcorrections than their corresponding control groups.

The elimination of errors of direction and of correction was due in part at least to the development of methods. At the beginning of practice a few subjects tried to use methods, but these were usually found ineffective. In the course of practice, methods were developed, however, and were used to eliminate the two sources of inaccuracy mentioned.

In adjusting to the reversing effect of the mirror several methods were reported. The majority of the subjects reporting methods found it best to correct an error kinæsthetically. That is, when a previous hit had been to one side of the target the subject tried to reverse the direction of his movement and go in the opposite direction on the next hit, the directions being determined kinæsthetically. Visual perception of errors played little part in this process, being used only to determine the existence and amount of errors but not their direction from the target.

A second method, used by a few subjects, was to try to go

further in the apparent direction of an error just previously made. Because of the reversing effect of the mirror this brought the subject back toward the target.

One further method was reported by two of our subjects, who observed that the side of the sheet of paper appearing further from the mirror was actually the side further away. That is, a hit at one side of the target might appear further from the mirror than the target itself and moving toward the mirror would correct in the proper direction. As this method was used by two subjects only, it was not very important in the experiment.

In order to overcome the tendency to overcorrect, a great many subjects reported that they made every corrective movement smaller than seemed necessary. Related to this method was the tendency (reported by nearly all subjects) to avoid trying to correct small errors. When a small error had been made, subjects found it better to refrain from any corrective movement and simply tried to repeat the previous movement.

One further factor that seemed of importance both in learning and in transfer was the initial nervousness and self-consciousness of some subjects. In many cases this was quite marked and was easily apparent to the experimenter. It is thought that this tended to decrease accuracy. After a few trials the nervous attitude disappeared and was replaced by an air of confidence. Usually, but not always, the confident attitude was carried over to practice with any other limb.

It was thought that an explanation of transfer might lie in the transfer of methods and in the elimination of the nervous attitude characteristic of so many subjects. Mere familiarity with the setting of the experiment might also be a factor in causing transfer. In order to test these various factors a new experiment was attempted in which the subjects were familiarized with the task and with the reversing effect of the mirror, and were given instructions as to the best methods to pursue in correcting errors.

On the spot usually occupied by the target a pendulum was set up. The screen was removed so that the pendulum

could be seen both directly and indirectly. The subjects were requested to watch the pendulum both directly and indirectly as it swung back and forth, alternating from one to the other method of watching. It was pointed out to them that the mirror reversed the direction of swing, causing the pendulum and its image to go in opposite directions, and that it also made the right side seem at the left and *vice versa*. After several minutes spent in watching the pendulum the subjects were shown their task by watching the experimenter go through the usual procedure of a trial with the right foot.

Following this demonstration the kinæsthetic method of correction was carefully explained to the subjects, who were requested to correct all errors by this method. They were also told to try to make smaller movements of correction than seemed necessary.

A third instruction was also given the subjects. Observation of the behavior of previous subjects had led to the conclusion that some of the errors were due to watching the foot as it approached the line. Subjects who did so would try to make corrective movements of the foot as they were hitting. This seemed to result in further error because the rhythm of hitting was lost, because it led to a jerky, uneven swing that was not conducive to accuracy and because errors of direction were frequently made. No subject reported this as a factor of importance, however, in the experiment. Consequently it may have been a mistake to instruct the new subjects to try not to correct on the forward swing.

The three points mentioned were explained carefully to the subjects and the experimenter made as certain as possible that the subject understood them. Ten trials of the foot were then given to each subject of the new group. There were ten men in this group. One of the ten consistently failed to follow instructions as to correcting errors of direction and stated that he found it impossible to do so. His results were extremely poor and he showed no improvement throughout the ten trials. The average of the other nine is compared in Fig. 7 with the average of all control subjects on the foot (Groups II and V) and with the average of all transfer subjects (Groups I, III and IV). It is shown that the instructed group is considerably better than the control group throughout the first ten trials. On the other hand, the new group does not show as great accuracy as the transfer

group for a few trials. The effect of the instructions and familiarity with the experiment has been to decrease considerably the early errors but not to the extent that they are decreased by previous practice with the right hand.

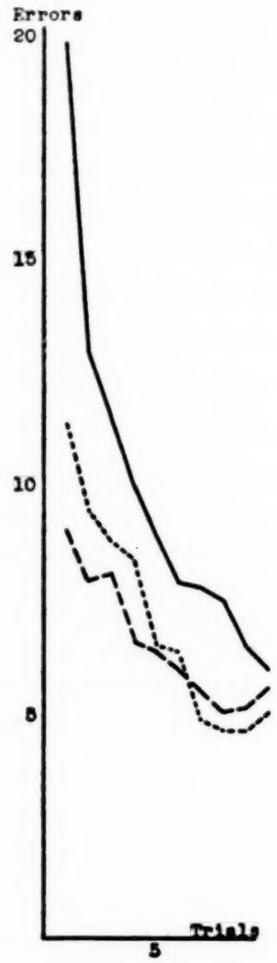

FIG. 7

——— Groups 1, 3, 4
——— Groups 2, 5
----- Group 8

There are several possible explanations of the differences between the instructed and the transfer groups. It may be that other factors than those on which instruction was given are present in transfer. Thus it was noted above that directional correction was often accompanied by movements of the whole body in the direction of correction. It is obvious that movements of this sort might play a large rôle in transfer.

Similarly there might be movements of the eyes or head which would aid the subject in learning with the foot.

It may also be, however, that the difference between instructed and transferred groups is due to a lack of ability

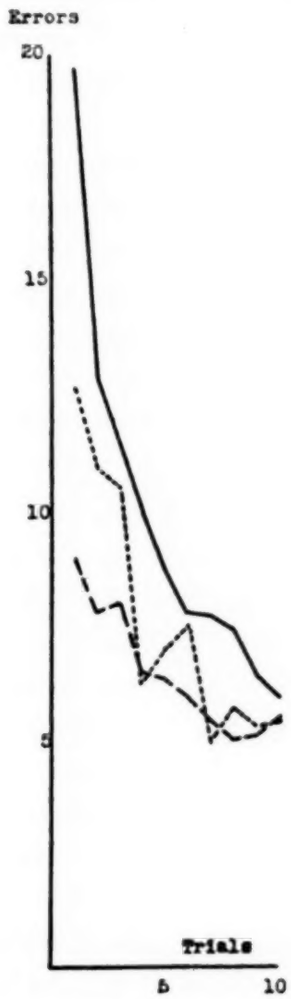

FIG. 8

——— Groups 1, 3, 4,
——— Groups 2, 5
- - - - - Group 9

to follow instructions given in a merely verbal way. Nearly every subject in the instructed group reported that occasionally it was impossible for him to follow the instructions. Methods which had been learned by actual practice of the individual himself would probably be more successful than those given to him in verbal form.

It was also thought that the third instruction might have influenced the results. The instructed group may have received an advantage from this that the transfer groups did not have. Consequently a new group of subjects was obtained and given the same instructions and demonstration that had been given Group VIII, except that the instruction with respect to correcting while on the forward stroke was omitted.

The results of this further work are shown in Fig. 8, where Group IX (the new group) is compared with the control and transfer subjects on the foot. This curve represents the results of seven out of the nine subjects in the new group. Two subjects found it quite impossible to follow instructions and were omitted from consideration. The average of the other seven again falls between those of the control and transfer groups. It shows a slightly greater error than that previously given for Group VIII, the other instructed group.

The results of this work with the instructions support the hypothesis that carrying over previously learned methods to practice with a new limb accounts for a great deal of transferred learning. This conclusion is likewise supported by an observation on one subject of Group I who adopted a new method when changed from hand- to foot-practice. The curve for this subject was quite unlike that for the average of the transfer group and resembled closely that of the control group.

On the other hand, as stated above, it is impossible to say that transfer of method is the only factor of importance in transfer. The instructed groups do not show as great accuracy as the transfer groups, and it is not known to what this difference is due.

Summary and Conclusions

It has been shown that in the mirror-accuracy experiment a transfer of learning from right hand to right foot takes place. The effect of previous practice with the hand is to increase considerably the initial accuracy of the foot and to save the equivalent of about six trials. It has also been shown that

varying the amount of hand-practice from ten to sixty trials has no observable effect on transfer to the foot. There are also indications that transfer does not occur if the first fall of the learning curve of the hand has not been completed.

Transfer from right foot to right hand also occurs. It seems to be less in amount but effective over a longer period than transfer from hand to foot. Likewise a slight transfer effect is observable from one hand to the other.

The explanation of this form of transfer probably lies, in part at least, in the transfer of methods which having been learned for one part of the body are carried over to practice with some other part. Mere familiarity with the setting of the experiment is probably also important in transfer.

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A STUDY OF THE CONDITIONED PATELLAR REFLEX

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The object of this investigation [1] was to determine the possibility of forming a conditioned knee jerk, and, if formed, to study the rate of its formation, its stability, and the effect of varying such factors in the procedure as length of training and the conditioning stimulus. Finally, it was desired to study the course of contraction in the conditioned and unconditional knee jerks and in the voluntary contraction of the quadriceps muscle-group.

The term 'conditioned reflex' was developed in Pavlov's laboratory as a name for the individually acquired connection, in an animal, between a stimulus and a response. Pavlov defines a reflex as "an inevitable reaction of an organism to an external stimulus, brought about along a definite path in the nervous system," and carefully shows that a conditioned reflex fulfills this definition. But "these new reflexes actually do depend on very many conditions, both in their formation, and in their physiological activity." Hence they are called 'conditioned' reflexes.[2]

The publication by Pavlov of an article entitled *Sur la sécrétion psychique des glandes salivaires* [3] is usually considered to be the beginning of the study of the conditioned reflex. Since that date (1904) there has been a constantly increasing interest in the subject. Pavlov has continued his work, and built up a school of Reflexology. He and his pupils have studied for over twenty years the cortical activity of the dog by the method of conditioned reflexes. They have linked conditioning stimuli from every sense department to either of two salivary reflexes. One of these unconditional reflexes is normally aroused by food and is an alimentary reflex, whereas the other is a response to acid placed in the mouth and is a mild defensive reflex. Pavlov says that in most cases the motor actions allied with these glandular responses are not taken into consideration, but are occasionally used when the results obtained from the secretion are not sufficient. An illustration of this resort to one of the motor components of an alimentary reflex is found in an investigation of the latent time of the conditioned reflex. Bykow and Petrowa measured the interval between the sounding of a bell (the conditioned stimulus) and the movement of the head toward the usual

[1] This study was carried out in the Psychological Laboratory of Princeton University.

[2] I. P. Pavlov, *Conditioned reflexes; an investigation of the physiological activity of the cerebral cortex*, 1926, (G. V. Anrep, tr., 1927), 23–25.

[3] *Arch. internat. de physiol.*, 1904, 1, 119–135.

source of food.[4] But Pavlov's system rests mainly on the study of conditioned salivary reflexes. He has summarized and organized the results of over twenty years of work in his book *Conditioned Reflexes*.[5] This is the first complete discussion of the work of Pavlov's school to appear in one of the more familiar languages, although several earlier and less complete summaries have appeared.[6]

Bechterev also made an extensive study of the conditioned, or, as he called it, the 'associative motor' reflex. He and his pupils have studied the conditioned withdrawal reflex using shock as the unconditioned stimulus, in both animals and man.[7] He differs from Pavlov in that his theories are applied to man, as well as to other higher mammals; whereas Pavlov usually confines his statements and theorizing to the acquired reactions of dogs.

In addition to the members of these two schools, there are many isolated investigators who have attacked the problem of the conditioned reflex in animals.[8] Most of them seem to have done only occasional pieces of work, and have not attempted a systematic formulation of the results. Beritoff is perhaps the most outstanding exception to this statement. He has verified most of Pavlov's results, using the flexion reflex of dogs as the unconditioned reflex. In addition to the movements of the stimulated leg, he recorded those of the head and of one other leg, thus obtaining a somewhat better picture of what the animal was doing. The most important aspect of Beritoff's work is his attempt to explain the conditioned reflex on a modern physiological basis. As he points out, Pavlov's explanations are not in strict accord with present physiological theory. But Beritoff is himself quite careful in his explanations not to ignore the results of those physiologists who are attacking the problem of the action of the central nervous system by methods other than that of the conditioned reflex.[9]

There has been considerably less investigation of the conditioned reflex in human subjects than in animals. Conditioned emotional reactions may be easily set up in infants,[10] as may feeding movements.[11] In adults, the iris or pupillary reflex has

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[4] K. M. Bykow & M. K. Petrowa, Die Latenzperiode des bedingten Reflexes (German summary of the Russian original). *Trudy physiol. laboratroyi akademika I. P. Pavlova*, 1927, 2, 147–149.

[5] Pavlov, *op. cit.*

[6] R. M. Yerkes & S. Morgulis, The method of Pawlow in animal psychology, *Psychol. Bull.*, 1909, 6, 257–273; S. Morgulis, Pawlow's theory of the function of the central nervous system, and a digest of some of the more recent contributions to the subject from Pawlow's laboratory, *J. Animal Behav.*, 1914, 4, 362–379; G. V. Anrep, The irradiation of conditioned reflexes, *Proc. Roy. Soc. Lond.*, 1923, 94B, 404–426; C. L. Evans, *Recent advances in physiology*, 1926; H. Cason, The conditioned reflex or conditioned response as a common activity of living organisms, *Psychol. Bull.*, 1925, 22, 445–472.

[7] W. Bechterev, *La psychologie objective* (N. Kostyleff, tr.), 1913.

[8] H. Cason, *op. cit.*

[9] J. Beritoff, On the fundamental nervous processes in the cortex of the cerebral hemispheres, *Brain*, 1924, 47, 109–145, 358–376; Ueber die individuell-erworbene Tätigkeit des Zentralnervensystems, *J. f. Psychol. u. Neur.*, 1927, 33, 113–335.

[10] J. B. Watson, *Behaviorism*, 2d ed., 1925.

[11] N. Krasnogorski, Ueber die Grundmechanismen der Arbeit der Grosshirnrinde bei Kindern, *Jahrb. f. Kinderhk.*, 1913, 78, 373–398; F. Mateer, *Child behavior; a critical and experimental study of young children by the method of conditioned reflexes*, 1918.

been conditioned to various stimuli,[12] as well as the eyelid reflex.[13] Bechterev, as mentioned above, has studied conditioned withdrawal from shock. Investigators in this country have established the same reflex.[14] The conditioned salivary reflex in man seems a little difficult to elicit in the laboratory.[15] This may be due to faulty technique, as Lashley supposes.

While studying the problem of facilitation of the knee jerk, Twitmyer[16] (1902) accidentally discovered that the knee jerk had become conditioned to the bell which he used as a preparatory signal. After this accidental discovery, he established conditioned knee jerks in six subjects. Dodge,[17] on the other hand, failed to establish conditioned knee jerks in several experiments. But Shevalev[18] apparently set up fairly stable conditioned knee jerks in seven adults.

The history of the 'simple' or unconditioned knee jerk is long and controversial. The knee jerk is the most easily elicited of the tendon reflexes, as well as the easiest to record. Since the tendon reflexes depend so largely upon the tonic condition of the muscle groups involved at the moment of stimulation, one of them, the knee jerk, has been used constantly by the physician in general diagnosis. This same dependence on tonic conditions makes it very sensitive to all other events in the central nervous system. For this reason the knee jerk has also been considerably used by the physiologist and the psychologist, especially in the study of facilitation and inhibition. Fearing has discussed this question, and has also written a brief history of the knee jerk from this aspect.[19]

A complete history of the knee jerk has not been written, nor for our purposes will it be necessary to do more than to summarize briefly the three views that have been held of the mechanism of the reflex.[20] These three views severally maintain as follows:

[12] H. Cason, The conditioned pupillary reaction, this JOURNAL, 1922, 5, 108–146.

[13] H. Cason, The conditioned eyelid reaction, this JOURNAL, 1922, 5, 153–196; R. Dodge, *Elementary conditions of human variability*, 1927, 68–69.

[14] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 1916, 23, 89–116; I. A. Hamel, A study and analysis of the conditioned reflex, *Psychol. Monog.*, 1919, 27, (no. 118), 1–64; H. Cason, *Psychol. Bull.*, 1925, 22, 459; G. Humphrey, The effect of sequences of indifferent stimuli on a reaction of the conditioned response type, *J. Abn. & Soc. Psychol.*, 1927, 22, 194–212.

[15] K. S. Lashley, The human salivary reflex, and its use in psychology, *Psychol. Rev.*, 1916, 23, 446–464.

[16] E. B. Twitmyer, *A study of the knee jerk*, 1902, Univ. of Pennsylvania.

[17] *Op. cit. inf.*

[18] E. Shevalev, The associative-motor reflex of the knee (Russian), (Bechterev 40th anniversary commemorative volume, 1926, 105–124). [Original not available, see *Psychol. Abst.*, 1927, no. 2158.]

[19] F. Fearing, A critique of the experimental studies of cortical inhibition with special reference to the knee jerk, *J. Comp. Psychol.*, 1927, 7, 285–296; The history of the experimental study of the knee jerk, *Amer. J. Psychol.*, 1928, 40, 92–111. See also W. James, *Principles of Psychology*, 1890, Vol. 2, 379–381.

[20] Since most of the references may be found in Fearing's history of the knee jerk only those not mentioned in that article will be given here.

(1) The knee jerk is a simple twitch of the muscle independent of nervous connections, except in so far as these connections determine the tonus of the muscle. The reflex can be facilitated or inhibited through these changes of tonus. Westphal, Gowers and Waller held this view.

(2) The knee jerk is a true reflex, of very short latency, involving (*a*) the proprioceptors in the muscle, (*b*) an afferent nerve, (*c*) a synapse in the cord, (*d*) an efferent nerve, and finally (*e*) the quadriceps muscle group. The controversy between the supporters of the first and second theories continued for some time. The strongest objection to the reflex view of the knee jerk was based on its very short latent period. It was thought that this latent time was too short for a true reflex. But careful determination of the latent time and analysis of the factors involved in it (Jolly, Snyder, Dodge, Hoffman) largely overcame this objection. Since then, other evidence in support of the reflex theory has accumulated as a result of investigation of (1) the course of contraction in the muscle (Dodge), (2) coördination of flexor and extensor in the knee jerk (Sherrington), and (3) the possibility of conditioning the patellar reflex to other stimuli (Twitmyer, Shevalev[21]).

(3) At present, the position that the knee jerk is a simple reflex is being abandoned, along with other so-called simple reflexes. The work of Sherrington, Liddell and Fulton,[22] and of Travis[23] especially, and the innumerable studies of the effect of facilitation and inhibition on the patellar reflex, make it apparent that the supposedly simple reflex not only involves a simple path from receptor to effector, with one synapse in the lumbar region of the cord, but also calls forth the activity of a fairly large portion of the central nervous system. The initial contraction of the quadriceps seems to be the result of a simple spinal reflex, but the curve of contraction of the muscle is soon complicated by discharges from higher centers.

It is, then, a complicated response that we are attempting to condition to antecedently irrelevant stimuli. There may be an objection to calling the result of such a conditioning a conditioned reflex; but in doing so, I am using the term in its conventional sense, with no implications in regard to the simplicity of the path of either the unconditioned or the conditioned knee jerk.

Preliminary Experiments

Preliminary experiments were made in order to explore the problem before the main part of the experimentation was attempted. Groups 1 to 6B were arranged, each planned after the completion of the preceding group, in a continued effort to obtain better conditioning.

The hammer used to elicit the knee jerk was of the usual pendulum type. The blow was delivered by the rounded edge of a wooden wedge which was mounted on a wooden block. This block served as the head of the hammer, and was mounted on a brass shaft in such a way that the striking edge of the wedge was at right angles to the patellar tendon. The shaft was a ⅜-inch brass rod, 25 inches in length. Since the

[21] *Op. cit.*

[22] J. F. Fulton, *Muscular contraction and the reflex control of movement*, 1926.

[23] L. E. Travis, W. W. Tuttle & T. A. Hunter, The tetanic nature of the knee-jerk response in man, *Amer. J. Physiol.*, 1927, 81, 670–678.

striking edge was an inch above the lower end of the rod, the hammer had an effective radius of 24 inches. The hammer was raised by hand before each blow and held in the raised position by an electromagnet. This magnet was mounted on a quadrant and could be raised or lowered as desired, thus changing the strength of the blow. It was never necessary to use more than 60 degrees to elicit a satisfactory jerk from any subject used. If this force was not sufficient, that obtained from a fall of 90 degrees also failed to elicit the knee jerk, and the subject was discarded. No attempt was made to determine the actual energy in the blow, since the sensitivity of the various subjects' knee jerks did not interest us. The weight of the shaft and head was two pounds. The quadrant, magnet, and the bearing on which the pendulum hammer swung, were all mounted on a sleeve, which was slipped over the rod of a 5-foot laboratory standard with tripod base, and held in position with two set screws, so that this whole unit could easily be adjusted for height. Adjustment in the horizontal plane was made by moving the standard. To make it possible to keep the support rod out of the way of the subject's leg when he kicked, without making it necessary to strike the tendon from one side, the quadrant, magnet and hammer bearing were mounted on the sleeve in such a way that the hammer fell in a plane 5 inches to one side of the supporting rod.

The time of fall of the hammer was carefully measured by kymographic technique (see Group 10, main series). It was found that, if the hammer was so adjusted that it hit the tendon at the bottom of its stroke, it struck .34 sec after its release. Due to unavoidable movements of the subject, and consequent slight changes of the position of his knee, this interval varied between .32 and .36 sec. No appreciable difference in this interval was caused by changing the height from which the hammer was released, since the period of the pendulum was not changed by a change in the amplitude of its vibration. This hammer was used throughout the experiments.

For the first three groups, the general set-up and procedure were as follows. The subject was comfortably seated on an elevated chair or bench, which had a broad desk-like arm attached to one side. The thighs projected several inches beyond the edge of the seat. An adjustable upholstered leg-rest was attached to the under side of the seat in such a way that it supported the stimulated leg just behind the knee. (With the exception of the last of the main groups this was the right leg.) The sides of this leg-rest prevented the leg from moving to either side. The other leg was supported by a foot-rest. A cardboard screen prevented the subject from seeing either the experimenter or the hammer. No kymographic record was made of Groups 1–3. The experimenter carefully observed whether or not the subject kicked when presented with the conditioned stimulus alone and also observed anything unusual, such as early kicks. Introspections were taken after practically every presentation of the conditioned without the unconditioned stimulus.

In Group 1, a 3-inch bell was used as the conditioning stimulus. Later it was replaced by one 6 inches in diameter, to give a louder sound. The bell was arranged in the following manner to give a single stroke when it was activated by the closing of the key. The clapper was so adjusted that it did not rest against the edge of the bell, but merely struck it once, due to the 'whip' of the clapper. This arrangement permitted the bell to reverberate until the key controlling the clapper was released, whereupon the bell was promptly deadened by a piece of sponge rubber, mounted on the clapper in such a way that the rubber rested against the edge of the bell when no current flowed through the coils of the bell. The interval between the bell and the blow was .34 sec, since the clapper, by means of a relay, was actuated by the same key that released the hammer.

The subject was seated comfortably and the hammer adjusted. He was permitted to read, or, if he preferred, to divert himself by drawing on the cardboard screen in front of him. Since all the subjects in this group, as well as the next three, were members of the faculty, graduate students, or seniors in the Psychology department, and were familiar with the conditioned reflex, no attempt was made to keep the purpose of the experiment a secret. They were instructed not to kick voluntarily at any time, but also not to attempt to inhibit a kick. The presentations of the conditioned and unconditioned stimuli were given in series of 5, with an interval of 5 sec between each double presentation, and of 15 sec between series. Fifteen such series were given the first day, making 75 double presentations in all, and 20 series on each of the succeeding sittings. Experimental periods were given to each subject once a week. The conditioned reflex was tested (presentation of the conditioned stimulus alone) once or twice toward the end of each period. It was feared that more frequent tests would wear out the conditioned reflex. Six subjects were given 9 experimental sittings each, totalling 875 double presentations.

Group 2 was designed to determine the effect of using various intervals between the conditioned and unconditioned stimuli. It differed from the preceding group in two ways. First, the double stimuli were no longer given in series but were presented at irregular intervals. The average of these intervals was 5 sec.

Secondly, the interval between the conditioned and unconditioned stimuli was not the same for all subjects. Each subject was assigned one of four intervals (.29, .34, .39, .44 sec), and this interval was used throughout all his sittings in this group. The interval was controlled by a motor-driven brass drum, with contact points that actuated relays controlling the bell and the hammer. Since the drum made one revolution in every 1½-sec it was necessary to insert a key in the circuit flowing through the drum, so that the relays would not be actuated every time the drum made contact. Thus the interval between presentations could be varied by omitting from 1 to 5 revolutions of the drum, while the interval between the conditioned and unconditioned stimuli remained unchanged.

Eight subjects were used, five of whom had been members of Group 1. From 2 to 5 sittings were secured from each. The group was discontinued before its completion, partly because several of the subjects were no longer available, and partly because the timing device was very irregular, due to fluctuations in the 110-volt alternating current that ran the drum.

Group 3 was arranged to test the result of a less remote, and presumably more compelling, stimulus. The conditioned stimulus was a loud click in a pair of earphones. It preceded the unconditioned stimulus by .34 sec. The earphones were connected in parallel with the coils of the magnet that supported the hammer. Hence there was a rather loud click when the circuit was broken to release the hammer, and a smaller one when it was re-established. In order to cause these two clicks to fuse into one sound, the circuit was interrupted only momentarily. This effect was secured by rapidly throwing over a double-throw mercury switch, with the two terminals connected together. Mercury contacts were used to eliminate the constant crackling that would have appeared in the phones if a metallic contact had been used.

Four subjects were used in this group, none of whom had had experience in a previous group. They were given respectively 2, 5, 5, 7 sittings (300 double stimulations to the sitting).

The method for Group 4 differed considerably from that previously used. The conditioned stimulus was a buzz in the earphones, which started .6 sec before the blow

occurred, and lasted until .1 sec after it. This buzz was produced by connecting the terminals of the phones in parallel with a small high-frequency buzzer. Although the buzzer was a very good one, it was found impossible to keep it vibrating at a constant frequency because of the wearing away of the contacts. A certain constancy was obtained by inserting a rheostat in series with the coils of the buzzer, so that the voltage might be increased to compensate for the wear on the contacts; but the device left much to be desired. It was not thought necessary, however, to install a vacuum tube oscillator as a more constant source of sound, in the preliminary work, since no attempt was being made to obtain differentiation of two unlike conditioning stimuli. The time relations between the conditioned and unconditioned stimuli were controlled by a pendulum, which started the buzzer as it commenced its stroke, released the hammer at the end of its single vibration, and stopped the buzzer as it completed its double vibration. A ratchet mechanism served both as a release to start the pendulum, and as a catch to stop it on its return. Since a certain amount of energy was lost in friction, the pendulum did not return completely to its original position, and therefore had to be run further up on the ratchet before it was released again.

After four subjects had each been given several sittings in this group, it was found that they were giving conditioned knee jerks with a fair degree of regularity. It was therefore decided so to modify the group that an objective record of these conditioned reflexes might be obtained. In Professor Dodge's discussion of the various means of recording the knee jerk,[24] he points out that the record of the thickening of the quadriceps muscle furnishes the most accurate description of the course of contraction of the muscle. Since one of the objects of this study was to compare the conditioned knee jerk with the unconditioned or 'simple' knee jerk, on one hand, and the 'voluntary' contraction of the quadriceps, on the other, it was decided to use this method. Since certain modifications have been made in the method as described by Dodge, to permit its use with a larger number of subjects, the arrangement used in this experiment will be described completely.

The subject was seated on a chair of such a height that the foot rested on the floor, without the flexor muscle of the thigh bearing the weight of the leg. The lower leg was held in position by being strapped against a V-shaped block, placed in front of the shin. A block of wood, $2\frac{3}{4} \times \frac{3}{4} \times \frac{3}{4}$-in was pressed against the quadriceps by the tension of two rubber bands stretched from the seat of the chair. The block was attached to a piece of copper wire .2 mm in diameter, which passed over three pulleys to a muscle lever. The light wooden pulleys had a V-shaped groove and revolved on hardened steel conical bearings. The muscle lever was of the usual type, with a cylindrical straw writing-arm and a parchment tip. As a counterpoise, a rubber band was used instead of the usual weight. The whole system was relatively free from inertia and consequent distortion of the curves. The lever wrote on a Ludwig-Baltzer kymograph, moving at a rate of 3 cm per sec. The hammer was arranged to fall so that a continuous record could be taken for 6–8 revolutions of the drum. The interval between successive double stimulations was decreased, so that it averaged 3 sec. Forty double stimulations could be recorded on each drum. During each experimental period two drums were used, the first recording presentations 1–40, the second, 200–240. Usually no tests of the conditioned reflex were made except while a record was being taken. Occasionally, however, tests were made at other times, or records were taken with no test, so that the sound of the kymograph would not acquire an inhibiting effect. In addition to the muscle lever, a signal marker was used to

[24] Dodge, R., *Zsch. f. allg. Physiol.*, 1910, 12, 1–58.

record the occurrence and duration of the conditioning stimulus (a buzz). Since the excursion of the signal marker lasted for .7 sec, it served as a rough time-line. No greater accuracy than this was desired in the preliminary work. The actual moment of stimulation of the knee jerk was furnished by a ripple in the record of the quadriceps, caused, as Dodge pointed out, by the mechanical stretching of the muscle.

Group 4 was continued, with the changes indicated, so that the conditioned knee jerks might be recorded. Six subjects were used, all but one members of a previous group. They were given from 3 to 11 sittings.

In the remaining groups, an extension kymograph was substituted for the one described above. The belt of paper used was 60 inches in length, and as only half of the belt was used at once, each belt could be used for two revolutions. Thus 120 inches of writing space were available. The kymograph was usually operated at a speed of 1 in. to 5 sec. This speed was sufficient to record clearly the height of the kick, and to show whether or not the subject kicked before the blow. When it was desired to study the curve of contraction, the movement of a lever increased the speed to the rate of 1 in. to .2 sec. The records were fixed by spraying them before removal from the drum. A (1 : 10) solution of shellac and grain alcohol, applied with a 'G.V' hand spray (designed for 'Duco,' etc.) was found very satisfactory. The kymograph permitted 10 min of actual recording, and as the interval between presentations averaged 3 sec, 200 double stimulations could be recorded on each belt. Since it was impractical to change the paper belt and smoke a new one while the subject waited, and since it was desired to record all presentations, it was decided to give only 200 double stimulations during each period.

In Group 5, it was planned to use a tactual stimulus as a conditioning agent. A soft iron stylus, with a felt-covered head ½-inch in diameter on one end, was used for the stimulus. It was 4 inches long, $\frac{3}{16}$-inch thick, and weighed 16 grams. The stylus was placed, head down, inside a piece of copper tubing that served as the core of a coil. When current flowed through the coil, the stylus automatically took a position in the core that was a resultant of gravity and the lines of force of the magnetic field. When the circuit was broken, the stylus dropped. A finger rest was arranged at a distance beneath the coil and stylus that would allow the stylus to rest on the finger after it had fallen 2 inches. The resulting tactual stimulation of the finger was the conditioned stimulus. The circuit through the coil was interrupted simultaneously with that through the hammer magnet, so that both stylus and hammer started their fall at the same moment. Since the stylus took a trifle over .1 sec to fall 2 inches, whereas the hammer took .34 sec to reach the tendon, the tactual stimulation occurred about .2 sec before the tap on the patellar tendon. In all cases (except tests of the conditioned knee jerk) the stylus was allowed to rest on the finger until the knee jerk had occurred. It was then lifted by closing the circuit. This same set-up was used for Groups 5, 6A, 6B and 7. Unfortunately, although the stylus itself was fairly noiseless, a relay and signal marker made audible sounds, so that the so-called tactual stimulus was really a combination of tactual and auditory stimuli. Five subjects were given 5 experimental periods each, on successive days, with 200 presentations to the period. Tests were given only on the last day, to be certain that we were not setting up the phenomenon Professor Pavlov refers to as 'internal inhibition.'

Group 6A was exactly the same as Group 5, except that the subjects were instructed to lift the stylus as soon as it fell on their finger, and to hold it in that position until after the blow. Certain important differences appeared when the results of this group were compared with the results of the earlier groups, but due to the fact that

very few tests were made in this group, it was felt that the results obtained should be verified by another group. Therefore group 6B was designed. It differed from group 6A only in certain differences of procedure. Printed instructions were given to the subjects, as follows:

1. Please get comfortable, seated well back in the chair, etc., before the adjustment of the apparatus. *After that, do not change your position unless you warn me before you do so.*

2. Do not allow anything to continue that is causing discomfort, no matter how slightly. The hammer should cause no pain as it falls, and the apparatus should not hinder the circulation of blood in the leg.

3. Report, at the end of the group, any irregularities that may occur in the procedure, if you notice them, *but do not introspect regularly.*

4. Place the middle finger of the right hand under the touch stimulus, so that it falls on the first joint from the end. Lift the stylus as soon as it falls.

After the instructions had been read, any unclear points were explained. The subjects were then told that there would be 20 presentations in each series, and 10 series during each experimental period. They were then given 20 'practice' presentations, during which only the tactual stimuli were presented. This was of course done only on the first day, and served to get the subjects used to lifting the stylus. If the record obtained during these practice presentations showed that the movement involved in lifting the stylus caused a movement of the quadriceps muscle, the subject was told not to lift the stylus so vigorously. After this preliminary series on the first day and on each of the remaining days, the subjects were given 10 series, during each of which the double stimulations were given 20 times. The conditioned reflex was tested during the first and second days, on the 12th stimulation of the third series; on the 13th and 17th, fourth series; on the 3d and 5th, fifth series; on the 9th, seventh series; and on the 4th, eighth series. To prevent memorization of this schedule of testing, it was changed on the third and fourth day to 16th stimulation, second series; 4th, fourth series; 18th, fifth series; 2d, sixth series; 14th and 19th, ninth series. Five subjects were used in this group, and were given 4 experimental periods each.

Discussion of Preliminary Results

Under Table 1 the results obtained from Groups 1 to 6B have been tabulated. Each subject in the preliminary

Table I

Preliminary groups, 1–6B; percentages (by subjects) of tests resulting in conditioned knee jerks

	Subject	A	B	C	D	E	F	G	H	I	J	K	L	M	N		Ave.
Group	1	8	0	25	50	35	0										20
"	2	9	50		14	0	0	0	59	0							17
"	3										55	13	21	17			27
"	4				11		0				33		9	25	0		13

	Subject	O	P	Q	R	S	T	U	V	W	X	Y	Z	A′	B′	C′	
Group	5	0	40	0	0	12											10
"	6A						16	67	0	100	67						50
"	6B											75	40	75	80	45	63

groups is designated by a letter, which he keeps throughout the experiment. Thus it may be seen, for example, that Subject D participated in three groups. The figures show the percentage of tests on which each subject kicked in response to the conditioned stimulus alone.

It may readily be seen from this table that, if some previously indifferent stimulus is repeatedly presented just before the knee jerk is elicited, eventually the presentation of this stimulus alone will call out the knee jerk. Twenty-one out of twenty-nine subjects kicked at one time or another when presented with the conditioning stimulus alone. This conditioned reflex was, however, extremely unstable. Only four subjects kicked on more than 67 per cent. of the tests, and only one reached a percentage of 100. Furthermore, these kicks varied in height from the smallest observable movement of the leg to a kick of approximately the height of the subject's unconditioned knee jerk. Examination of the records of the conditioned knee jerk showed that they differed considerably from the unconditioned reflex. Since no time line was used in the introductory groups, these differences cannot here be stated accurately, and will not be discussed until later.

In Table I the percentages of trials on which each subject showed a conditioned knee jerk have been averaged. These averages appear at the right of the table and furnish a means of roughly evaluating the efficiency of the conditions in each group in forming a conditioned knee jerk. It will be noted that the averages of Groups 1–5 vary between 10 and 27 per cent. (For the present, the results of Groups 6A and 6B will be omitted from the discussion.) Since there are so few subjects in these groups, none but the most pronounced differences may be considered significant. Therefore, any conclusions drawn from these results must be considered only as suggestions for future experimentation.

In the first place, it would seem that the interval between successive double stimulations does not make much difference in the rate of formation of the conditional knee jerk. Group 1, in which this interval was 5 sec, shows about the same conditioning as Groups 2, 4, and 5, in which the interval between presentations of the double stimulation was only 3 sec. Furthermore, variation of interval between the conditioned and unconditioned stimuli seems to make no great difference within the limits tested. Group 5 is about as effective as Group 4, although the interval in the former group was .2 sec and in the latter, .6 sec. These two groups in turn are about as effective as Group 1, in which the interval between bell and blow was .34 sec. Furthermore, in Group 2, where several intervals were used for the various subjects, no pronounced difference in favor of any of the intervals was seen.

Consideration of the original records by successive days (not included in the table) makes it apparent that the number of repetitions has a limited effect. The frequency

with which any one subject gives conditioned reflexes does not always increase from day to day. One subject (A), for example, who gave a strong conditioned knee jerk during the first sitting, gave none after that. It was decided to plan the experiments in the main group so that they might throw light on the effects of frequency in the formation of the conditioned knee jerk.

No decisive difference was noted in favor of any of the first five groups, with the possible exception of 3. Only four subjects were used in this group, and the average was raised from 17 per cent to 27 per cent by one exceptionally good subject, so this difference is not particularly significant. It also seemed that the conditioning was about as effective when the leg was prevented from moving (Groups 4, 5) as when it was allowed to swing freely (Groups 1, 2, 3).

By referring to the results of Group 6A, it may be seen that the percentage of conditioned reflexes obtained in this group was about twice as great as that of any of the earlier groups. It will be noted that this group differed from 5 only in that the subjects always responded to the conditioning stimulus in the former group by lifting the tactual stylus, whereas they did not make any voluntary response to the conditioning stimulus in Group 5. Yet Group 5 was somewhat less effective than the other preliminary groups. Since very few tests were made of the conditioned knee jerk in either of these two groups, little significance could be attached to this difference. Yet in view of the fact that Group 5 gave about the same results as the other groups in which no voluntary response was used, it was felt that the results for this group were fairly valid. To give the results of Group 6A an equal validity, Group 6B was arranged, as noted above. It may be seen from the results of this group (Table I) that the percentage of conditioned knee jerks obtained was again twice that obtained from any of the non-facilitated groups; that is, those in which no voluntary response was used. Thus the results obtained from Group 6A were verified, and it seemed quite likely that the increased effectiveness of conditioning in these groups was due to the effect of facilitation. It was thought best, therefore, to plan the main groups so that they would show the effects of facilitation by a voluntary response to the conditioning stimulus.

Main Experimental Groups

The results obtained from the preliminary groups cannot be considered of great validity, owing to too large a number of uncontrolled factors, and to too few subjects. Indeed, it must be remembered that these groups were merely exploratory and were not expected to yield more than suggestions for further study. The main groups were planned to furnish more conclusive data, so that fairly reliable conclusions might be drawn. To make the groups comparable, they were all built on the same pattern. Since the pattern used in Group 6B was found quite satisfactory as regards frequency and number of double presentations, instructions, frequency of tests of the conditioned reflex, etc., this pattern was adopted for the main groups. Nine or ten subjects were used in each group, and no subject was a member of more than one group,

nor had had previous training in a conditioned reflex experiment.

The first of the main groups (Group 7) involved facilitation of the reflex by a voluntary lifting of the stylus, as had 6A and 6B. Instead of lifting the stylus every time it touched his finger, as in the previous groups, the subject lifted it only 50 per cent of the time. The instructions were changed to read "Lift stylus only on trials 1, 3, 5, 6, 8, 12, 13, 15, 17, 18" for the first two sittings; but for the third and fourth this was changed to 1, 2, 4, 7, 10, 11, 15, 16, 17, 19 to prevent memorization of the order. The subject was allowed, of course, to keep the list before him during the experimental period. He was required to make the count himself, and was instructed to report if he lost it, whereupon the correct count would be given him. Nine subjects participated in the group, each of whom had four sittings, with the exception of two subjects who had only three. In all other details, the group was just like group 6B.

In Group 8 the conditioning stimulus was a bell, arranged as in Group 1, so that it gave a single stroke .34 sec before the blow. In place of lifting the tactual stylus, the subjects were instructed as follows: "As soon as the bell rings do the following.—On trial 1, clench both fists. On trial 2, do nothing. On 3, say 'ah'; 4, nothing; 5, 'ah'; 6, fists; 7, nothing; 8, 'ah'; 9, nothing; 10, nothing; 11, nothing; 12, fists; 13, 'ah'; 14, nothing; 15, nothing; 16, nothing; 17, fists; 18, 'ah'; 19, nothing; 20, nothing," and on the third and fourth days this order was changed to "Trial 1, clench both fists; 2, clench left fist; 3, do nothing; 4, left; 5, nothing; 6, nothing; 7, nothing; 8, nothing; 9, nothing; 10, left; 11, left; 12, nothing; 13, nothing; 14, nothing; 15, right; 16, left; 17, right; 18, nothing, 19, left; 20, nothing." There were nine subjects, each of whom had 4 experimental periods. Except for the above differences, the procedure in this group was the same as that used in Group 7.

Group 9 was designed to determine the minimal latent period of the conditioned knee jerk. It was feared that the intervals between the conditioned and unconditioned stimuli used in all the previous groups were too long to permit the formation of a conditioned reflex of a very short latency. Therefore the conditioned and unconditioned stimuli were made more nearly simultaneous, by decreasing the interval between bell and blow to .04 −.11 sec. The bell was mounted on the head of the hammer and swung with it, to determine whether the subjects would then identify the bell more closely with the blow. A contact on the hammer brushed another contact on the support thus sounding the bell just before the hammer reached the tendon. The contact was adjusted for each subject so that he reported that the bell preceded the blow. The interval between bell and blow was then determined by the kymographic record. The hammer was always operated by hand to eliminate any noises that might be made by the depression of a key. These noises would always have preceded the blow by several tenths-of-a-second, so that the effective interval would have been that between the release of the hammer and the blow, rather than that between bell and blow. To insure a blow of constant strength, the hammer was always raised to the (now-inactive) magnet as a stop. When it was desired to make a test of the conditioned reflex, the hammer was moved rapidly past the contact by hand, but was not permitted to touch the patellar tendon.

It had been planned to put the subjects through the usual four sittings, as in Group 8, and then if any of them developed fairly stable conditioned reflexes, careful records were to be taken of the unconditioned and conditioned knee jerks, as well as of the voluntary contraction of the quadriceps. These records were to be taken with

the kymograph moving at the rate of 1 inch in .2 seconds. The single electric marker was replaced by a triple marker. One of these markers was connected in parallel with the coils of the bell, another was connected to improvised keys around which the subjects were instructed to clench their fists, and the third was wired in parallel with the coil of a 50 d.v. electrically maintained tuning fork. Thus there was available a record of the moment of occurrence of the conditioned stimulus, the unconditioned stimulus, the movement of the quadriceps, the clenching of the fists, and finally, a time line.

Since none of the subjects in this group showed more than an occasional conditioned reflex, the desired records were not obtained. Therefore several subjects that had been members of previous groups, and had developed very stable conditioned reflexes, were given several sittings in this group. One of them developed a stable conditioned reflex after 200 double stimulations, and records were obtained of the conditioned reflex, the unconditioned reflex and the voluntary contraction of the quadriceps.

Group 10 was designed primarily to test the effect of stimulating both legs simultaneously, instead of only one, and of permitting them to swing freely, instead of holding them rigid. With the exception of certain modifications in the apparatus made necessary by these two changes, this group was similar in all details to 8, so that the results of the two groups are strictly comparable. One other minor difference in Group 10 was the use of the 'fist keys' described in Group 9, so that a record of the clenching of the fists might be obtained if desired.

The subject was seated in the chair used in Groups 1, 2, and 3, modified by the addition of another leg rest, so that both legs could be simultaneously stimulated. Another hammer was mounted on the sleeve of the stimulating apparatus, opposite the original hammer. The magnet support was lengthened and another magnet added, to hold the new hammer in the raised position. The new hammer head could be moved a short distance on its shaft and was held in place with a thumb screw, so that it could be adjusted to small differences in height between the two legs. The stimulating unit was placed in front of the subject, with the supporting rod in such a position that the subject's legs swung on either side of it when he kicked. Since both hammers had to be caught and cocked by hand, it was necessary to mount the telegraph key which released them on the floor where it could be operated by the foot of the experimenter. The addition of a heavy spring made a standard telegrapher's key sufficiently stiff to be worked satisfactorily with the foot. Recording was still done by the method of muscle thickening, as before. The response of the right leg alone was recorded. The first part of the curve resembled that obtained with the leg held rigid, but the last portion was distorted by movements of the lower leg. This movement of the leg usually started before the muscle had reached its maximal thickening.

Three out of the nine subjects used in this group developed stable conditioned reflexes by the end of the fourth sitting. A new smoked belt was then put on the kymograph, and several conditioned and unconditioned knee jerks were recorded, using the technique described in Group 9.

Results and Discussion of the Main Experimental Groups

The results of the main experiments, as summarized in Table II, are as follows.

TABLE II

MAIN GROUPS 7, 8, 10; PERCENTAGES OF TESTS RESULTING IN CONDITIONED KNEE JERKS (SUBJECTS 70–98)

Group 7. Recorded by muscle thickening

Facilitation Employed	Subjects									
	70	71	72	73	74	75	76	77	78	Ave.
Lift Stylus	58	56	46	35	13	71	39	15	58	43
None	50	30	14	14	15	10	11	11	0	17

Group 8. Recorded by muscle thickening

	Subjects									
	80	81	82	83	84	85	86	87	88	Ave.
Clench Fists	17	80	100	75	75	50	75	100	100	75
Say "Ah"	20	60	56	20	71	33	50	83	83	53
None	50	0	25	11	33	13	80	33	0	27
Clench R Fist	60	50	0	33	33	100	50	100	50	53
Clench L Fist	67	11	33	83	20	100	100	83	0	55

Group 10. Recorded by muscle thickening

	Subjects									
	90	91	92	93	94	95	96	97	98	Ave.
Clench Fists	75	75	40	100	100	0	100	25	50	63
Say "Ah"	83	67	40	100	20	17	33	50	17	47
None	0	38	63	100	38	22	38	12	25	37
Clench R Fist	0	67	50	100	50	100	50	0	50	52
Clench L Fist	16	80	100	100	67	71	50	50	33	61

Group 10. Observed movement of lower leg

	Subjects									
	90	91	92	93	94	95	96	97	98	Ave.
Clench Fists	25	0	60	100	50	0	100	50	25	46
Say "Ah"	0	0	20	100	0	0	17	33	0	19
None	0	0	50	100	38	22	25	0	13	28
Clench R Fist	0	33	50	100	50	100	50	0	0	43
Clench L Fist	0	0	83	100	67	71	33	33	0	43

(1) In several groups or sub-groups,[25] the subjects gave conditioned knee jerks in over 50 per cent. (average) of the tests. All of the subjects in Groups 7, 8, and 10 gave conditioned knee jerks at one time or another when they were presented with the conditioning stimulus alone. The results of Group 9 are not presented in the table, for reasons that will be explained later. The results of this experiment, therefore, are in agreement with those of Twitmyer,[26] who was able to set up a conditioned knee jerk in all six of his subjects,

[25] By sub-group is meant those double stimulations which are followed by a given facilitating response, as 'clench fists' or 'ah,' *e.g.*, in Group 8.

[26] E. B. Twitmyer, *A study of the knee jerk*, 1902.

and with those of Shevalev,[27] who was successful in conditioning the reflex in seven subjects. Dodge[28] reports that he failed to get a "reconfiguration of the mechanism of the knee jerk" in several situations that might have been expected to produce a 'conditioned knee jerk' (although he does not use the term). The situations in Dodge's experiment were somewhat different from the present, a fact which may explain the difference.

Unlike Twitmyer's, however, our subjects failed to get stable conditioned reflexes. One-third of the subjects in Groups 7, 8, and 10 showed complete conditioning in one or more sub-groups, *i.e.*, gave a conditioned knee jerk on every test. From an examination of the daily records of each subject (not included in this report), it was found that only three of the twenty-seven subjects used in the main groups kicked on every test in any sitting, if we consider all subgroups instead of just one. Furthermore, most of the conditioned reflexes were very small in extent. In fact, in Groups 7 and 8 most of them measured under 1 mm on the record. Explanations of this instability will be suggested later.

Introspections[29] as to the nature of the knee jerk varied considerably. Although several of the subjects felt that it was partly or completely voluntary, the vast majority were quite certain of its involuntary nature. Many of the subjects, especially in the groups in which the leg was allowed to swing freely, expressed surprise when they felt the leg move without the usual blow. One subject (No. 93) was unable to inhibit the conditioned knee jerk, after he had asked permission to do so, even when warned before the test that the blow would not be given. This evidence of the involuntary nature of the conditioned knee jerk is given, not because of its intrinsic value—indeed, the term 'voluntary' might well be kept out of a discussion of the conditioned reflex—but because this evidence shows that the subjects were not consciously motivated to kick when the conditioned stimulus was presented. Additional proof of this lack of motivation is given by the fact that almost all the subjects in the main groups had no idea of what a conditioned reflex was, and were kept in complete ignorance of the nature of the experiment until after it was completed.

In addition to the actual knee jerk, there were many other indications of con-

[27] E. Shevalev, *Bechterev 40th anniversary commemorative volume*, 1926. (See footnote 18.)

[28] R. Dodge, *Elementary conditions of human variability*, 1927, 68, 69.

[29] The subjects in Groups 1–4 had had some training in introspection, whereas most of those in later groups were untrained. The introspections given by the former class of subjects differed chiefly from those of the latter in their completeness.

ditioning. Chief among these was the tendency to kick before the blow occurred. This happened fairly frequently. Several other subjects showed a slight withdrawal of the knee just before each blow, although they reported that the tap on the tendon was not at all uncomfortable. Finally, those subjects who were practiced in introspection (members of the preliminary groups) reported 'tension,' 'kinæsthesis,' 'tingling,' and similar phenomena, all located in the stimulated leg, when the conditioned reflex was tested by the presentation of the conditioning stimulus without the blow. Several other subjects reported that they had kicked, after a test, even though the leg had not actually moved at all. All these reports suggest that something was happening in the muscles of the leg. Unfortunately no records of the quadriceps thickening are available for these subjects, since the preliminary groups in which they participated were not recorded on the kymograph. The only similar phenomena reported in the main groups was a 'desire to kick.' This seemed uncorrelated with any movements of the recording lever, therefore any associated muscular responses must have been of the nature of tonic changes.

(2) *A.* In determining the latent period of a conditioned reflex, two possibilities present themselves. We may obtain the average of several measurements, as Cason did [30] in his work with the wink reflex, or we may take the shortest latent time obtained. Bykow and Petrowa [31] favor the latter method, on the ground that repetition of the conditioned stimulus without reinforcement by the presentation of the unconditioned stimulus increases the latent time. Since each measure has something in its favor, both will be given here.

Subject B′, who was the only one to form a stable conditioned knee jerk after training with the short bell-blow period used in Group 9, developed a conditioned knee jerk with an average latency of .47 sec. When he was told to kick voluntarily as soon as the bell rang, the period was the same; *i.e.* he kicked .47 sec after the bell rang. The latent time of his unconditioned knee jerk was about .04 sec. One could not expect the latent period of the conditioned reflex to be less than .1 sec, since during his training period the knee jerk had always followed the bell after an interval of .1 sec. The shortest latent period, however, was .32 sec. This was longer than the shortest reaction time of voluntary quadriceps contraction recorded for this subject, which was .26 sec.

Latent periods of the conditioned knee jerk and of the voluntary contraction of the quadriceps muscle are available for three subjects from Group 10. Here the period between bell and kick during the training period had been .35 sec. The latent periods are as follows:

[30] H. Cason, The conditioned eyelid reaction, this JOURNAL, 1922, 5, 153–196.

[31] K. M. Bykow & M. K. Petrowa, Die Latenzperiode des bedingten Reflexes (German summary; see footnote 4).

Subject 92. Latent period of conditioned knee jerk, average of two determinations, .40 sec. Minimum .40 sec. Voluntary contraction of quadriceps, .40 sec.

Subject 93. Latent period, conditioned knee jerk, average of 11 determinations, .34 sec; minimum, .20 sec. Voluntary contraction of quadriceps, average of 5 readings, .46 sec. Minimum, .22 sec.

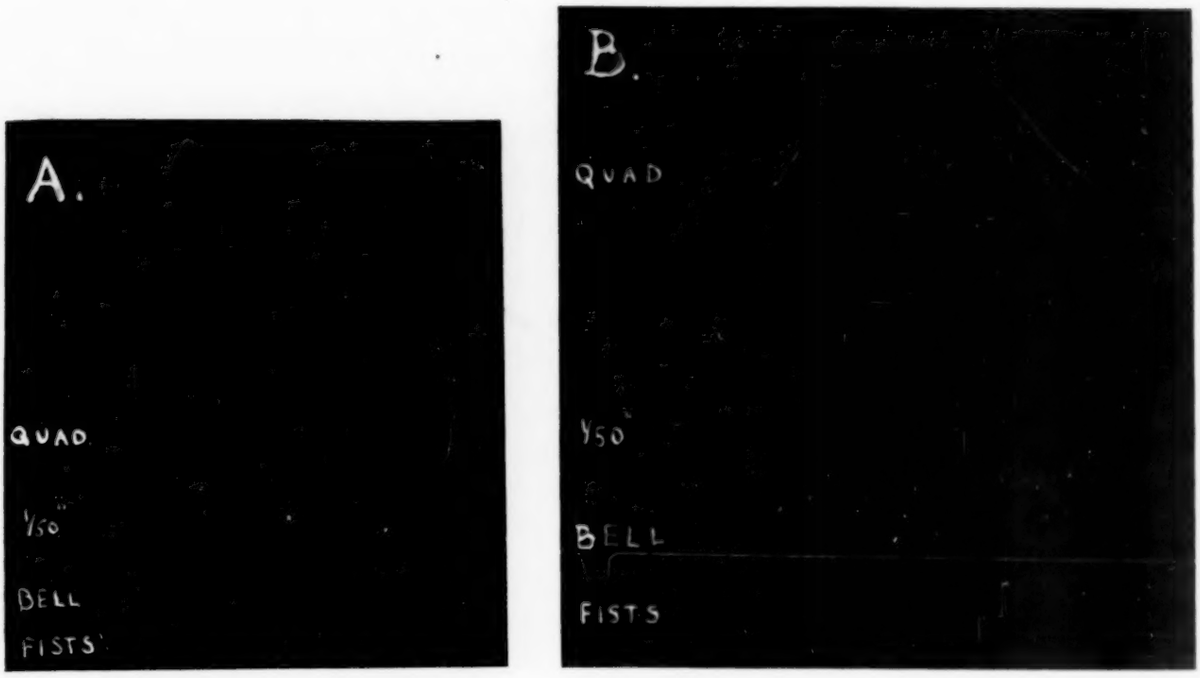
Subject 95. Latent period, conditioned knee jerk, average of 7 determinations, .28 sec; minimum, .16 sec. No voluntary contractions recorded.

It may be seen from these figures that the latent time of the conditioned knee jerk in four subjects averaged between .28 sec. and .47 sec. This latent period was about the same as that required for a voluntary kick, and about ten times the latent period of the unconditioned knee jerk. This result is in agreement with those of Humphrey,[32] who finds the latent period of the conditioned withdrawal of the hand from shock to be .3–.4 sec. The latent periods obtained in this experiment are longer than those obtained by Dodge and Cason for the conditioned eyelid reflex, or by Bykow and Petrowa for the flexion reflex of the dog.

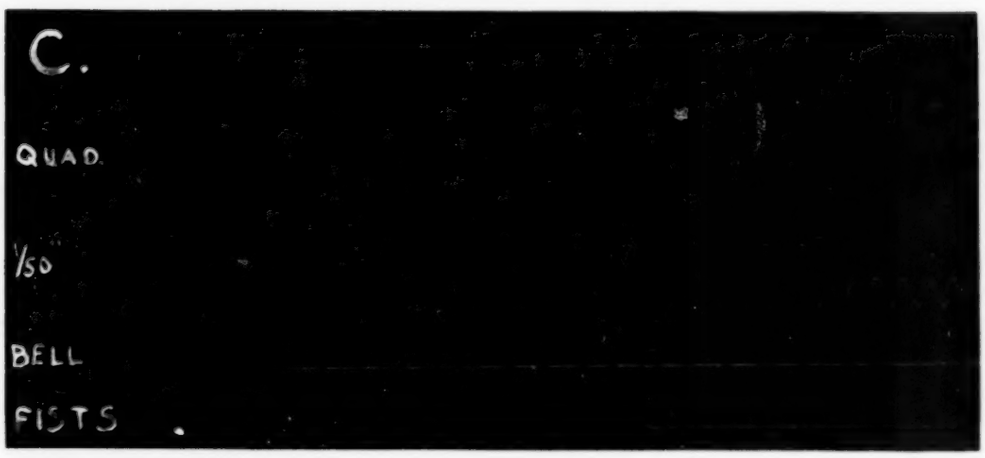
B. The drum record below (Fig. 1) represents an unconditioned knee jerk, a voluntary contraction of the quadriceps, and a conditioned knee jerk. These were all obtained during one experimental period from subject B′ who had developed a stable conditioned reflex. It may be seen from these records that the curve obtained from the conditioned reflex is very similar to that obtained during voluntary contraction of the quadriceps and quite different from the record of the unconditioned jerk.[32a] The most striking difference between these two curves and that of the unconditioned knee jerk is the sharp initial rise seen in the latter. This difference is found in all the records of the conditioned and unconditioned knee jerks

[32] G. Humphrey, The effect of sequences of indifferent stimuli on a reaction of the conditioned response type, *J. Abn. & Soc. Psychol.*, 1927, 22, 194–212.

[32a] The initial movement of the recording lever that shows on the record of the unconditioned knee jerk as a ripple is not due to an actual contraction of the muscle but to the purely mechanical stretching of the muscle by the depression of the tendon. It marks the actual moment of stimulation of the end organs in the muscle.



A. The Unconditioned Knee Jerk
B. Voluntary Contraction of the Quadriceps



C. The Conditioned Knee Jerk
Quad. = Thickening of the Quadriceps
$\frac{1}{50}''$ = Time line (50 d.v. fork)
Bell = Moment of completing circuit through electric bell
Fists = Voluntary clenching of the fists

FIG. 1

that I have obtained. A second difference shown in the records is the greater rapidity of fall in the case of the curve of the unconditioned knee jerk, when compared with the other two records. This rapid fall, although very frequently found, is not always seen in the records of the unconditioned knee jerk, for occasionally the return to the base line is quite as slow as it is in records of the conditioned reflex.

After noting the apparent similarity between the conditioned and the unconditioned knee jerks, Twitmyer[33] said that "if differences really exist, a much more rapid recording device will be needed to detect them." He recorded the knee jerk by attaching a pen, its carriage and a weight, to the heel of the subject by means of a string, and then permitting the pen to write on a moving paper. This method of recording, owing to its inertia, would obviously fail to show the rapidity of the first initial rise that is so characteristic of the unconditioned knee jerk, and so notably lacking in records of the conditioned knee jerk.

As noted in the introduction, Fulton has discussed at great length[34] the effect of the higher centers on the later phases of the unconditioned knee jerk; and the work of Travis and his associates[35] shows that the action currents of the quadriceps found during the later phases of the knee jerk resemble very closely those obtained from the same muscle during a voluntary kick.

These findings support the conclusion drawn by Dodge [36] from his work with reflexes that a simple reflex usually initiates movements; but that these movements are promptly taken over and modified by 'voluntary' or cortical control. If we apply Dodge's view to the unconditioned knee jerk, it would seem, then, that the resemblance between it, on the one hand, and the conditioned reflex and the voluntary contraction of the quadriceps muscle, on the other, is due to the presence of impulses from higher centers, whereas the difference between them is caused by the short reflex through the lumbar region of the spine, which occurs in the unconditioned knee jerk, but not in the conditioned reflex or in the voluntary contraction. Thus a consideration of the curve of the conditioned reflex and of its latent time supports the widely prevalent view that conditioned reflexes involve higher centers.

[33] E. B. Twitmyer, *A study of the knee jerk*, 1902.

[34] J. F. Fulton, *Muscular contraction and the reflex control of movement*, 1926.

[35] L. E. Travis, W. W. Tuttle & T. A. Hunter, The tetanic nature of the knee jerk response in man, *Amer. J. Physiol.*, 1927, 81, 670–678.

[36] R. Dodge, Protopraxic and epicritic stratification of human adjustments, *Amer. J. Psychol.*, 1927, 39, 145–157.

(3) The results of Group 9 have not been presented in the tables in percentage form, as have the other groups, because of a certain difficulty that developed in scoring them. There were very few movements of the muscle that could clearly be ascribed to the presentation of the conditioning stimulus. In seven of the ten subjects, whenever the period between one double stimulation and the next was more than the average (3 sec), the muscle would thicken in little jumps. The record looked very much like a flight of steps. It seemed as if the subject were anticipating the blow and contracted the quadriceps before it arrived. This thickening usually occurred at a more rapid rate and reached a higher level when the conditioned stimulus was presented without the unconditioned during the thickening. This step-like rise had been noticed occasionally in three subjects in earlier groups, but had occurred only when several tests were presented in succession. It seems probable that, in this series, the subjects were becoming conditioned to the interval between double stimulations, as well as to the bell. It is likely that this change in conditioning was caused by the extreme shortness of the interval between the bell and the blow. In this group the interval bell-blow was never more than .11 sec. This is less than the shortest latent time obtained for the conditioned knee jerk. It may be that reducing the interval between the conditioned and the unconditioned stimulus to a value below that of the latent period of the conditioned knee jerk has an effect somewhat similar to that produced by reversing the order of stimulation.

(4) Table III gives the percentage of conditioning from day to day. All the facilitated sub-groups of Groups 7, 8

TABLE III

THE EFFECT OF FREQUENCY

Sitting	1	2	3	4
Facilitated	54	60	50	50
Unfacilitated	11	23	38	38

The figures indicate the average percentage of conditioned knee jerks obtained in the facilitated and unfacilitated sub-groups, respectively, computed for each sitting.

and 10 are averaged to obtain the upper row of percentages; while the unfacilitated sub-groups of these groups are averaged to furnish the lower row. It may be seen from these averages that the conditioning in the facilitated sub-groups very rapidly reaches a maximum for given conditions; whereas the maximal conditioning is delayed until the third experimental period in the unfacilitated sub-groups. The decrease in the percentages for the third and fourth days of the facilitated sub-groups may have been due to the change from facilitation by clenching both fists, or by saying 'ah,' to clenching the right or left fist (Groups 8 and 10); but the fact that a similar decrease in percentage of conditioned reflexes is seen where the same facilitation is continued throughout (Group 7), is rather against this hypothesis. It is possible that the decrease is due to a diminishing effectiveness of the facilitation, through a gradual automatization of the voluntary facilitating response.

(5) From the results and from the previous discussion it may be seen that the conditioned knee jerk in man, as obtained in these experiments, is very instable. The cause of this instability is not known, but several theories may be suggested.

A. In the first place, most conditioned reflexes in man are somewhat unstable. Humphrey[37] reports some difficulty in setting up a conditioned withdrawal of the hand from shock, and Watson[38] had trouble in forming a similar withdrawal of the foot. Cason[39] found it difficult to establish a conditioned lid reflex in certain subjects; although the conditioned stimulus was a sound just too weak to call out the lid reflex before the training had taken place. Several investigators found no conditioned salivary reflex in man, but Lashley[40] reports that they may be set up quite easily in the laboratory, if conditions are properly controlled. Various investigators have suggested that the reason conditioned reflexes are hard to establish in man is that he is less easily deceived than animals. Lashley, in discussing the failure of others to elicit a conditioned salivary reflex from man, states this objection in more objective terms. He says that "the stimuli to conditioned secretion in man are complex situations which are not duplicated under laboratory conditions" (459). It will be noted that difficulties arising from lack of control of the conditions were carefully eliminated

[37] G. Humphrey, *J. Abn. & Soc. Psychol.*, 1927, 22, 194–212.

[38] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 1916, 23, 89–116.

[39] H. Cason, The conditioned eyelid reaction, this JOURNAL, 1922, 5, 153–196.

[40] K. S. Lashley, The human salivary reflex and its use in psychology, *Psychol. Rev.*, 1916, 23, 446–464.

from the present experiment. Since the subject could not see the experimenter, and since the slight movement of the experimenter's hand in holding the hammer gave the subject no auditory cues, he had no means of telling beforehand when the blow was to be omitted, or, in objective terms, there was no stimulus that could set up conditioned inhibition.

B. Another possible explanation of the instability is found in the fact that the unconditioned knee jerk undergoes great variations in strength, so that one knee jerk may be quite vigorous and the next one almost lacking. Pavlov has pointed out [41] that the strength of a conditioned reflex varies with the strength of the unconditioned reflex on which it is based. This is probably one of the factors involved in the instability of the conditioned knee jerk. For if the condition of the organism at a given moment were such that the unconditioned knee jerk would have been weak, the conditioned reflex, if tested at that moment, might be so weak that it would not objectively show. It is likely that this factor is not the only one, for some subjects either kicked vigorously or not at all when presented with the conditioning stimulus alone. If the variations in the strength of the unconditioned reflex were the sole explanation of the instability of the conditioned reflex, we should not expect to find subjects who either kicked vigorously or not at all, but only those whose conditioned responses varied by small steps between zero and a maximum in extent.

C. There was one very obvious inhibition present in this experiment which may have been partly responsible for this instability, namely, the withdrawal from the blow. Every effort was made to prevent any discomfort from this source, and all subjects reported that the blow was not uncomfortable. It was noticed, however, that several subjects, especially those who had weak patellar reflexes, and consequently needed a stronger blow to elicit the reflex, developed a tendency to withdraw the knee slightly before each blow. Several of the more candid subjects reported that although the blow was not painful, it was still somewhat of a shock, and that they might have been withdrawing from it. One subject (F) who regularly moved his leg to the left, instead of kicking, when the reflex was tested, felt that this movement was a resultant of tendencies to kick forward and to withdraw from the blow. It seems quite probable, therefore, that the instability of the conditioned knee jerk may have been partly due to an inhibition arising from the shock of the blow. There is a possibility that the fact that the leg was held rigid may have been another inhibitory factor. This will be discussed under 7.

D. Reflexes, in addition to their objective strength, show another quality that has been called 'prepotency,' 'physiological strength,' etc., referring to the ability of that reflex to inhibit other opposed reflexes when both are stimulated at the same time. Thus, in the decerebrate dog, the flexion reflex will inhibit the scratch reflex. This quality of a reflex is said to correlate with its 'biological utility.' The physiological basis of this prepotency is not known, for its explanation apparently involves a knowledge of the mechanism of inhibition. But as a generalization obtained from the observation of reflex behavior, this 'prepotency' seems valid.

Pavlov has pointed out [42] that this physiological strength or biological importance (to use his terms) determines the ease with which a reflex may be conditioned. The apparent lack of this 'biological importance' in the knee jerk is quite apparent. Indeed, Dodge has referred to it as 'an apparently imbecilic reflex.' [43] Furthermore,

[41] Pavlov, *Conditioned reflexes, etc.*

[42] *Op. cit.*, 30, 31.

[43] R. Dodge, Problems of human variability, *Science*, 1924, 59, 269.

the ease with which the knee jerk is facilitated or inhibited shows how little prepotency it possesses. Accepting Pavlov's generalization, then, it is possible that this lack of physiological strength, whatever its physiological mechanism may be, is one of the factors causing difficulty in establishing a stable conditioned knee jerk.

Too little is known about the mechanism of the central nervous system to make a further attempt at explaining this instability of the conditioned patellar reflex of any value. All of the factors mentioned above, and perhaps others, may be involved. But enough has been said to show that the conditioned reflex is not always the simple product of simultaneity of stimulation and frequency of repetition that so many assume it to be.

(6) When we compare the sub-groups in which facilitation was used with the unfacilitated, in Groups 7, 8, and 10, with regard to the percentage of tests in which the subjects showed conditioned knee jerks, an interesting fact may be observed. If we consider the records of these groups as obtained by recording muscle thickening, we find that no one of the unfacilitated sub-groups is as effective as the poorest of the facilitated. This difference is especially significant, since there were 3 unfacilitated and 9 facilitated sub-groups. Furthermore, the unfacilitated sub-groups involved 4 times as many double stimulations as did any one of 8 of the facilitated, and the same number as the ninth. Any practice-effect, therefore, would favor the unfacilitated sub-groups. In spite of this, the average percentage of conditioning for the 9 facilitated sub-groups is 56 per cent.; whereas that for the unfacilitated is only 27 per cent. If the preliminary groups are included in this average, the difference becomes more striking, namely 57 per cent. compared to 21 per cent.

It was felt that this difference might be due to the inclusion within the percentages of the facilitated sub-groups of many small movements of the record that were actually caused by movements of the whole body, rather than by contractions of the quadriceps. The subjects were all requested not to make the voluntary response so energetically that it caused a twitch or vibration of the whole body. As noted under Group 7B, all subjects were watched for this movement during the 'practice series' that preceded the regular series on the first day. Furthermore, if any subjects were suspected of making such movements during the course of one of the sittings, they were requested to clench the fists and the like, in response to the command of the experimenter, while he watched the record for such movements. One subject was discarded because he did not seem able to eliminate these movements. To test the question further,

however, all the records of Groups 7 and 8 were re-examined, and scored for those movements only that showed over 1 mm on the record. The increased effectiveness of the facilitated sub-groups showed just as clearly, even under these conditions; although practically all of these small bodily movements would have been eliminated by this method of scoring. The average for the facilitated sub-groups, when scored in this way, was 27 per cent., whereas that for the unfacilitated was only 6 per cent.

As another check on the existence of this difference, Group 10 was reconsidered. It will be remembered that the leg was allowed to swing freely in this group. In addition to the kymographic records of muscle thickening, there was available a set of notes, taken by the experimenter during the sittings, as to whether or not the subject actually kicked on each test. These data have been tabulated in Table II (Group 10). It may be seen that only one facilitated sub-group; namely, that in which the subject said 'ah,' is exceeded by the unfacilitated sub-group. This reversal is caused by the fact that this type of facilitation was used only on the first two days. This sub-group did not benefit, therefore, as much from practice as did the unfacilitated sub-group. The percentage obtained from the 'ah' sub-group is half again as large as that obtained from the unfacilitated sub-group on these two days.

We may safely conclude, therefore, that the occurrence of a voluntary response, made to the conditioning stimulus during the conditioning period, increases the effectiveness of the conditioning. It may be seen from Table III that this facilitation effects the conditioned response in two ways; namely, by decreasing the number of double stimulations needed to produce the maximal conditioning for the given situation (the maximum is reached on the second day in the facilitation sub-tests, and on the third in the unfacilitated) and by increasing the extent to which the knee jerk may be conditioned. Although the complete explanation for this increased effectiveness is not known, several hypotheses present themselves.

A. The effect of 'attention.' The preparation to give a voluntary response, such as clenching the fists, as soon as the conditioning stimulus is received, directs the attention toward this stimulus. This would probably give the stimulus a greater effectiveness, and therefore better and more rapid conditioning might be expected.

Furthermore, attending to the stimulus and the voluntary response would take the attention from the unconditioned stimulus and response. Certain subjects reported that they gave the highest and most frequent conditioned reflexes when their attention was directed toward their reading matter, or when they were 'lost in thought.' Stated in

physiological terms, this may be the removal of the inhibitions mentioned above.

This explanation has many points in its favor. Pavlov found that excitable dogs formed conditioned reflexes most rapidly.[44] He also found that, under certain conditions which made the dogs more excitable, all inhibitions vanished. These two observations seem to be borne out to some extent by the results of this experiment. In addition to the attentional effects noted above, it was noticed that the subjects who gave the best conditioning were of the quick 'nervous' type. Furthermore, several subjects who normally gave no trace of a conditioned knee jerk gave frequent large reflexes upon becoming emotionally disturbed during the course of an experimental period. This disturbance took the form of amusement at some humorous idea that occurred to them or of embarrassment at being late for the sitting or disturbing the recording apparatus by a careless movement. Since no objective measurement of 'nervousness' or of emotional disturbance was at hand, only the observations of the experimenter can be offered as evidence. It is hoped that further work can be done to investigate this matter, both in the conditioned knee jerk and in other problems of the conditioned reflex type.

B. Another possibility is that the voluntary response facilitated (in the physiological sense) the conditioned knee jerk. This facilitation may be directly effective on the conditioned reflex, or indirectly through increasing the strength of the unconditioned reflex. The tonus of the whole body would be increased by the 'set' of the subject to clench his fists, etc., as soon as the bell was heard. This increased tonus might well be accompanied by a lowered resistance in the nerves supplying the quadriceps. There would thus be a greater likelihood that a weak excitation would cause the muscle to contract.

C. There is the further possibility that the facilitating response acts as an additional unit in the constellation of stimuli that becomes the conditioned stimulus situation for the conditioned knee jerk. The voluntary response almost always occurred during or just before the contraction of the quadriceps. The fact that the effects of the voluntary response were so marked, whereas other changes in the conditioning stimulus seemed to have little effect, makes this possibility somewhat unlikely. Another point against it is the fact that those subjects who developed the most stable conditioned reflexes usually made the movement of the quadriceps and the voluntary response at the same instant. It has been noted that it is very difficult to set up a condi-

[44] *Conditioned reflexes, etc.*

tioned knee jerk when the interval between the conditioned and unconditioned stimuli is very short. Hence the facilitating response probably occurs too late to act as a conditioning stimulus.

(7) The effects of permitting the leg to move freely, as in Group 10, rather than holding it in a rigid support (as in the earlier groups) were somewhat unexpected. As may be seen from Table II (Group 10), this technique had no great effect on the percentage of conditioned reflexes when the group is considered as a whole. It did tend slightly to decrease the differences between the sub-groups; although this result may have been obtained by chance, since it is based on nine subjects only. The most pronounced effect, however, was a very great increase in the height of the conditioned contraction. Whereas most of the conditioned reflexes obtained in the earlier groups were under 2 mm, as measured on the record, more than half of the conditioned knee jerks obtained in Group 10 were over 2 mm. When the leg is permitted to swing freely during the knee jerk, there is a large mass of stimuli caused by this movement, coming from muscle, tendon, joint and skin. These stimuli, and the reflexes that they excite, apparently account for part of the contraction of the quadriceps muscle, particularly in the later phase of contraction. When the leg is kept from moving, stimuli from the skin and joints are lost, together with their reflexes. Without these components, one would expect the conditioned knee jerk to be smaller than it would have been had they been retained by permitting the leg to swing freely as in Group 10. It is probable, then, that the increased height of the conditioned knee jerks obtained with the leg swinging freely is due to the proprioceptive reflexes, which are not present when the leg is prevented from moving.

Another possible factor presents itself, namely, that inhibitions are set up when the leg is kept from swinging. This factor probably does not play an important part; for, if it did, the frequency of the conditioned knee jerks would increase, as well as their height, when this inhibition was eliminated. As noted above, there is apparently little, if any, increase in the frequency of conditioned reflexes obtained in Group10. This same objection may be raised against the possibility that the increased height obtained in this group is caused by the fact that both legs were stimulated, and that the simultaneous contraction of the muscles of both legs acted as mutual facilitation.

Summary

1. Conditioned patellar reflexes have been formed, using a bell, a click, a buzz, and a tactual pressure as the conditioning stimuli.

2. These conditioned knee jerks have been formed in 44 out of 49 subjects.

3. When the interval between conditioned and unconditioned stimuli was varied between .20 and .44 seconds there was no significant difference in the ease of conditioning; but when this interval was decreased below .11 seconds it was more difficult to form a conditioned reflex.

4. The conditioned knee jerks formed were very unstable, varying considerably in both height and frequency of appearance, even within one experimental period.

5. Subjects varied considerably, both in their rate of conditioning and in their ability to form a conditioned knee jerk.

6. Computed by groups or sub-groups the percentage of trials on which conditioned knee jerks appeared varied between 10 and 75.

7. When facilitation, in the form of a voluntary response to the conditioning stimulus, was used, conditioned knee jerks were obtained more than twice as frequently as when no facilitating response was made.

8. The records of the conditioned knee jerk resemble those of the voluntary contraction of the quadriceps group, and they both differ from those of the unconditioned knee jerk, chiefly in that the sudden initial rise seen in the latter is not found in the former.

9. The latent period of the conditioned knee jerk varies between .2 and .5 sec, being about the same as that of the voluntary contraction of the quadriceps group, and 5 or 10 times that of the unconditioned knee jerk.

10. When the lower leg is allowed to swing freely, conditioned reflexes are obtained about as frequently as when the leg is not permitted to swing; but the conditioned reflexes obtained in the former situation are much bigger than those obtained in the latter.

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EMOTIONAL CHARACTER OF THE SINGING VOICE

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The following experiment[1] was designed to discover the extent to which an observer is able to discern emotional characteristics in the adult voice when the characteristics of sung tones are kept constant and visual clues are eliminated. In addition to a knowledge of the outside conditions which cause the vocal reaction of an individual, the casual observer commonly employs a great variety of clues, such as the intensity, pitch and duration of the sounds, the facial expression, movements of the body and the bodily reaction in general. The conditions of this experiment did not allow the observers to employ any of these indicators of emotion.

The observers were thirty graduate students in psychology whose reports were obtained in the following manner. S, an accomplished singer, was asked to denote a given emotion by presenting a single note (duration about 1½ sec) repeated five times. The intensity and pitch were held as constant as possible, and the vocalist was wholly screened from the observers. The note was sung on the fundamental *e* with the syllable "Ah," at intervals of 1 sec between the five repetitions.

The emotions which the vocalist intended to convey were surprise, fear-pain, sorrow and anger-hate. After the note had been repeated five times, an interval of about a minute was allowed for the observer to write the name of the emotion which the note seemed to him to express. The observers were told beforehand that the vocalist would attempt to convey four different and distinct emotions, but these were not named.

In order to determine the influence of pitch upon the judgments of the observers the series of notes was repeated on the fundamental *a*. The observers were merely told that another group of tones would be presented and that they were to name the emotions which the tones seemed to them to represent.

Table I gives the reports of the 30 observers of notes sung upon *e* and upon *a*. In the *e*-series, 18 emotions were named where the vocalist intended but 4. 18 of the 22 reports of 'sorrow' given were made in response to the notes intended to convey sorrow. Of the 18 reports of 'anger-hate,' 15 were returned from the anger-hate notes of the singer. Surprise and fear-pain were less well conveyed. 10 of the 20 observers who made judgments of the sounds intended to manifest fear-pain named these emotions, the other 10 naming 8 other emotions. To the sounds intended to convey surprise only 3 of the 21 observers so reported.

In the *a*-series (also Table I) only the pitch was changed, the note now being based upon another fundamental tone. 14 emotions were reported, the order of frequency being sorrow, anger-hate, fear-pain, and so on. 22 of the 28 reports of 'sorrow' given were made in response to the notes intended to convey sorrow, and 16 of the 18 reports of 'anger-hate' likewise accorded with the vocalist's intention. Surprise and fear-pain, as in the *e*-series, were less well conveyed. Of the 21 observers reporting a judgment of the notes intended to convey surprise, not one observer

[1] From the Psychological Laboratory, University of Chicago.

detected this quality, and only 7 of the 17 giving a report of the notes intended to convey fear-pain referred to this quality.

TABLE I

EMOTIONS INFERRED FROM A NOTE SUNG UPON FUNDAMENTALS *e* AND *a*

	Emotion intended by the vocalist									
	Surprise		Fear-Pain		Sorrow		Anger-Hate		Totals	
Pitches	*e*	*a*	*e*	*a*	*e*	*a*	*e*	*a*	*e*	*a*
Emotion reported										
Pleasure	1				1				2	
Fear-pain	7	1	10	7	1	2	1		19	10
Desire					1				1	
Pugnacity							1		1	
Bravado	1								1	
Mirth			1						1	
Sorrow	1	4	2	2	18	22	1		22	28
Exultation	1						2		3	
Daring	1								1	
Displeasure			1				1		2	
Horror				1	1				1	1
Help wanted			1		1				2	
Distress	2								2	
Anger-hate	1	2	2				15	16	18	18
Surprise	3		1						4	
Anxiety		2	1	2	1	1			2	5
Antagonism							2		2	
Longing					1				1	
Defiance								2		2
Excitement		2		2						4
Peace		2								2
Reverence						2				2
Defeat				1						1
Confidence								5		5
Sorrow or joy		1						1		2
Regret						1				1
Dread		2				1				3
None	3	5	1	2	2			2	6	9
Totals[2]	21	21	20	17	27	29	23	26	91	93

It appears that the change in pitch from *e* to *a* did not materially alter the judgments. In both series the reports of sorrow and anger correspond closely to the emotions which the vocalist intended to convey; there was less correspondence with fear-pain, and little with surprise.

In order to ascertain whether a melodic sequence of notes is emotionally significant, the notes were combined into a melody. The melody chosen was made up of the following notes, each with a duration of 1½ seconds:

a b c b a b c d e d b c a.

The melody was first presented as a whole in the key of A-major and then repeated in the key of A-minor, and the observers were asked to name the emotions which these melodies conveyed to them. After that only the first five notes of the melody were presented, first in A-major and then in A-minor. The conditions of the presentations were in all respects similar to those employed in the experiment with the single notes.

[2]Not all observers made reports upon each vocal presentation, being unable to make a decision in some cases.

Table II gives the reports of the observers of the melodies. 42 reports were given in response to the longer melody, in contrast with 34 to the shorter. That the longer melody was more effective is also indicated by the fact that only three reports of 'none' (*i.e.* no emotion) were given to the longer melody and 9 to the shorter.

TABLE II

EMOTIONS INFERRED FROM MELODIES

Emotion reported	Long melody		Short melody		Totals			
	A major	A minor	A major	A minor	Long Melody	Short Melody	A Major	A Minor
Sorrow	3	9	5	6	12	11	8	15
None	3		4	5	3	9	7	5
Confidence	3	2	2		5	2	5	2
Religious reverence	4				4		4	
Joy with reverence	2	1	1		3	1	3	1
Monotony		2	1	1	2	2	1	3
Fear		2	2		2	2	2	2
Relaxation	1	2		1	3	1	1	3
Joy	3				3		3	
Expectation	1	1			2		1	1
Disagreement			2			2	2	
Sympathy		2			2			2
Love			1	1		2	1	1
Anger	1			1	1	1	1	1
Regret			1			1	1	
Totals	21	21	19	15	42	34	40	36

Sorrow was named more often than any other emotion, and more often in response to the minor than to the major key. When the longer melody was observed, 9 reports of sorrow were given to the minor key and only 3 to the major. This difference was not apparent in the responses to the shorter melody. No significant differences are evident in the relation between key or length of melody and any of the other emotions reported.

SUMMARY AND CONCLUSIONS

The emotional character of the adult singing voice can sometimes be apprehended from the qualitative character of the voice, without the aid of outside clues. The reports of a group of graduate students in psychology upon the emotional character of the voice of a trained vocalist, when the intensity, duration and pitch of certain notes were kept constant and visual clues eliminated, showed a close correspondence with the emotions which the vocalist intended to convey when sorrow and anger-hate were in question. Fear-pain was less well conveyed, and there was little correspondence with surprise. 18 emotions were named from notes sung upon the fundamental *e* and 14 from *a*, where the vocalist intended but 4. The reports were not materially altered by the change in pitch.

A sequence of notes, whether they form a melody or merely repeat a single note, may convey emotional significance to the listener. Sorrow was named more often than any other emotion; and more often with the minor than with the major key. Other emotions named showed no significant relation to length of melody or to the character of the key.

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